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THE CLINICAL IMPORTANCE OF FIBROSITIS IN GENERAL PRACTICE*

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Everyone will agree that any contribution to the understanding and treatment of pain is of fundamental importance in practice. My remarks are directed solely to one cause of pain, that arising from some pathological condition of, or injury to, white fibrous tissue. There is need for much clearer clinical appreciation of white fibrous tissue pain. Such a conception should be taught more systematically in the schools and described more clearly in the textbooks. The word "fibrositis," connoting a definite pathology, has only come into anything like general usage in the last twenty years or so, and even now its implications are not fully realized. Only the other day I came across a remark made by a distinguished surgical colleague to the effect that he was rather sceptical about the condition (fibrositis), saying that "it was one physicians found, but surgeons rarely found it."

When this tissue undergoes pathological change (either acute or chronic inflammation, injury, or chronic strain) it becomes *painful*; and having regard to its widespread presence in the body it must be obvious that many cases of pain can be explained in this way. Not only does it become painful, but it tends to undergo an inflammatory proliferation of varying type and degree, forming either linear, stringy thickenings, thickened plaques or sheets, or fibrous lumps or nodules; the last consisting of fibrous septa enclosing muscle fibres and bundles of characteristically vague and ill-defined outline. The pain is variable; it may be continuous or intermittent, dull and aching or sharp and paroxysmal, sometimes of knife-like intensity, even mimicking true trigeminal neuralgia; with a notable variability under conditions of bodily health and weather. From these facts it is clear that the basic conception of a white fibrous tissue pathology is essential for the analytical study of pain in general. I want to emphasize that until this fundamental idea is fully grasped not only will many everyday conditions escape accurate diagnosis, but many serious diagnostic errors will continue to be made.

Certain special methods of clinical investigation are required. White fibrous tissue is to a very large extent accessible to direct palpation, and it is in these more accessible areas that its pain-producing pathology is liable to become manifest. The first point to be realized is that when fibrous tissue has undergone these pathological changes it is *always more or less tender to pressure or strain*, and this even when it is not actually giving rise to pain apart from pressure or strain. Strain may be caused by any form of pulling and dragging, and therefore the pain is particularly liable to be associated with movement.

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Dr. Stockman[1] has pointed out that the pathology of "toxic" fibrositis consists in a swelling between, and a proliferation of, the tissue fibres, together with changes in the finest vessels and nerve endings. The small blood vessels are distinctly thickened in all their coats, and therefore the affected area is liable to congestion and consequent increased irritation of the inflamed nerve endings. This fact explains why the pain is frequently so much worse when the patient is warm in bed, tending to get better after being up for some time.

The term "fibrositis" is coming into more general adoption, but the condition has, of course, received other names in the past. Most commonly it has been called "muscular rheumatism"; inaccurately, for it is neither muscular nor rheumatic, though there are close anatomical and clinical associations. The fibrous tissue, in its septa, sheaths, and tendons, being affected, many of the resulting thickenings and swellings are felt to be in, and therefore thought to be of, the muscular tissue itself, to which the pain is not unnaturally but erroneously referred. "Neuritis" is another popular term, and is only accidentally and partially accurate; its frequent use seems to depend in part on its having a "superior" sound as compared with the commonplace and old-fashioned term "muscular rheumatism."

The true rheumatic infection, particularly in younger subjects, is a common cause of fibrositic pain. This is clinically recognized as the "growing pains" and other aches of the rheumatic state in childhood. Yet it is important to emphasize that, curiously, the true rheumatic nodule (itself almost reserved to childhood) is not only different in anatomy, pathology, and distribution from the nodular formations of the fibrositic condition, but is still further differentiated by being absolutely painless, even on palpation. We may have, therefore, in true rheumatism a definite nodular formation which has nothing to do with the rheumatic pain, as well as true secondary fibrositis (in early life due to the rheumatic poisoning), and this is also to be distinguished from the articular manifestations of the disease.

Aetiology

The aetiology, as might be expected, is complex. There are three main factors to be recognized.

1. *Septic and Toxic Influences.*—Among these influenza is common and important, and the widespread pains during the height of the disease and frequently persisting afterwards are due to this cause. The reaction of the white fibrous tissue to gonorrhoea is well known, and as has already been pointed out, it is a secondary result of the rheumatic infection. It is found in many other toxic and infective conditions, ranging from oral sepsis to

erythema nodosum. Gout undoubtedly predisposes to white fibrous tissue pain.

2. *Injury*.—This may be acute in any form of injury by contusion or laceration, and this type of fibrositis presents certain differences, both of pathology and of localization, from the infective and toxic types. Yet it calls for inclusion under the general term "fibrositis," because the repair processes of the damaged tissue (due to an ordinary leucocytic exudate which is not present in the other forms) gives rise to the characteristic pain. In the more chronic trauma caused by strain we have a very large group of cases. The strain may be single, as from the act of winding up a car or lifting a weight, or repeated and persistent from the adoption of certain strainful attitudes. The most typical strain manifestation is seen in ordinary lumbago.

3. *Climate*.—The variability of fibrositic pain with the weather has long been known; indeed, some people are so sensitive as to be almost human barometers. There are personal differences, however; not everybody reacts in the same way to a particular change of weather. Usually the pain is intensified before the advent of rain rather than with its onset; and exposure to cold east wind, etc., as preceding or accompanying an attack is too well known to need emphasis. The exact mechanism by which atmospheric conditions operate is obscure, in spite of their undoubted and frequent incidence.

Special Clinical Manifestations

If the basic conception of white fibrous tissue pathology and pain, as briefly outlined, is thoroughly understood the various clinical manifestations cannot easily escape recognition. But there are some which deserve special mention, either on the grounds of frequency or importance, or amenability to treatment; and it is remarkable that some of them have for so long escaped their full meed of recognition.

FIBROSITIC HEADACHE

This is one of the most striking examples of the truth of the preceding sentence. Even to-day I doubt if the majority of practitioners, general or special, know what is meant by the term "fibrositic headache." Attention was first drawn to it by the Scandinavians, particularly in their schools of massage, and when the exponents of this treatment came to this country (incidentally teaching us most of the practical massage we now know) they were laughed at for saying they could cure headache by massaging the back of the neck. I laughed myself. But because I have made it an invariable rule never to reject without examination anything seriously maintained by intelligent people, I was led to a full understanding of the clinical importance of the "white fibrous tissue pathology."

It is the commonest cause of chronic continuous or intermittent headache, yet sufferers from it are still sent in their thousands to the ophthalmic surgeons, who seem to know less about it than anybody else. It has characteristics which are well defined, enabling a diagnosis to be made with easy accuracy. The headache is recurrent and persists for varying periods, from a few hours, days, or weeks, and varies much in intensity from time to time. It is often worse in the morning, passing off as the day goes on; although quite frequently it develops towards evening. In the first case the aggravation is due to the congestion which occurs during the night; and in the second to the effects of strain (and possibly other causes) during the day. It is essentially a "nape-of-the-neck" pain, concentrated in the basi-occipital area and extending down the muscles at the back of the neck, and thence becoming in many cases a generalized headache. The whole fibrous area of the scalp may be tender and painful; this is frequent in the temporal regions and the fibrous tissue round the supra-orbital nerve, tending to grip it with thickened fibrous tissue as it goes through the notch; if unilateral it may have a distribution mimicking true hemicrania.

In every case of persistent headache this cause should be borne in mind and investigated. When palpation-pressure is applied to the tissues at the base of the skull its aponeurosis and tendons arising therefrom are found to be tender to such pressure, sometimes exquisitely tender, even when at the moment no headache is being experienced. Now a normal nape is not tender in this way. This tenderness is of a variable extent and distribution; it is most commonly found in the situations I have indicated, and also extends down the fibro-muscular tissues of the cervical spine and into the muscles of the supra-spinous fossae. Very often these areas are quite obviously thickened or nodular to the skilled touch (it must be a skilled touch), and more or less well-defined infiltration can be noted in the suboccipital and other muscles.

The attention to the four diagnostic features—persistence, thickening, nape-of-the-neck localization, and tenderness on pressure—will enable the diagnosis to be made with certainty, though it must be remembered that other causes of headache may also be present and must be borne in mind and investigated—for example, renal disease and high blood pressure. Most marvellous relief from the proper treatment is possible; indeed, if these remarks were of no further service than to help the recognition of the frequency and curability of this type of headache, while it would doubtless deprive ophthalmic surgeons of some part of their practice, patients would benefit considerably.

"TRIGEMINAL" NEURALGIA

Even less known than fibrositic headache is the fact that there are many cases so closely resembling true trigeminal neuralgia as to receive this diagnosis. I desire to emphasize that it is as imperative to investigate such cases from the point of view of fibrositic pathology as in chronic headache. However severe and paroxysmal the pain may be, if on palpation of the muscles of the jaw and face one finds the characteristic reaction of pain on pressure, treatment by (skilled) massage should certainly be tried. I have now had so many cases yield to this line of treatment that I am coming to the conclusion that there are more cases of fibrositic "trigeminal" neuralgia than of true trigeminal neuralgia.

CHEST CASES

These are extremely interesting, and their recognition will enable many minor and a few major diagnostic errors to be avoided, as well as procuring gratifying relief to the patients. First of all the rib cases. The white fibrous tissue of the ribs and their cartilages and the muscular attachments thereto are prone to localized and often quite small areas of fibrositic thickening (whether perceptible as such or not) and tenderness. Owing to the continuous respiratory movements the pain is very frequently of a continuous kind, though modified by respiratory movement. I have known three cases of chest pain (two of them in doctors of eminence) in which a definite diagnosis of aneurysm was made; yet all the trouble was due to a small tender fibrositic spot in or near the aortic area, yielding the characteristic definite and localized pain on palpation, which cleared up with a few days' massage. The error was due to the particular localization combined with lack of knowledge of "rib fibrositis."

A very large number of cases are diagnosed as heart disease when these pains occur in or near the praecordia. Many are due to strain conditions (often forgotten by the

patient) affecting the pectoral muscles, which is not surprising. In women the pain sometimes leads to a suspicion of cancer, but unfortunately if the true nature of the pain is not recognized relief is unlikely to follow. Most cases of pleurodynia are of this nature, and one need scarcely comment upon the fact that no rub is heard. I regard this as a serious diagnostic error, because on the known fact that most pleurisy, in the absence of any other definite cause, is tuberculous in pathology, so to label a case is a serious matter ; almost as serious as failure to realize the tuberculous pathology of " idiopathic " pleurisy.

Heart disease, again, is a label which is often so applied by the doctor, and still more frequently by the patient, to these simple cases. One of the earliest examples of this in my experience was a young policeman who had complained of persistent pain about the lower end of the sternum, which led to his being invalided from the force with the label of " heart disease." The heart was perfectly normal, but he had marked localized fibrositic tenderness of, and near to, the xiphisternum, which very rapidly and completely cleared up under massage, leaving him grateful but grumbling, though these emotional reactions were differently directed.

ABDOMINAL CASES

In this field a knowledge of fibrositic pain is particularly useful, as the incidence of such cases is frequent and often puzzling. The fibrous tissue connected with the abdominal muscle sheaths or attachments is particularly liable to be affected in this way. Should the pain be located, even approximately, in the right lower quadrant of the abdomen, a patient is lucky to escape the surgical knife. Diagnoses of gastric ulcer and cholecystitis are sometimes made with a certainty which is only rivalled by the negative findings at operation.

The characteristic pressure pain, which is such a valuable criterion when palpating over a hard bony surface such as the nape of the neck or the ribs, is a factor that is much less reliable over the abdominal wall ; sometimes, indeed, a certain diagnosis cannot, or should not, be made. Far better that an abdomen should be opened with a negative result than that cases of organic visceral disease should be overlooked with possible resulting catastrophe. It is obvious that fibrositic pain is more superficial because of its parietal origin ; whereas pain of visceral origin will be deeper. There is one method of palpating the abdomen in pursuit of this differential diagnosis that has very largely escaped notice and certainly systematic teaching and demonstration. I refer to the difference in pain results from what I call " finger-tip " pressure as compared with " flat-hand " pressure. From the localization of the painful tissue in the abdominal wall it is first and most directly accessible to pressure by the exploring hand. In the true fibrositic cases this pain is readily elicited by the focalized searching-out pressure of the tip of a finger ; with the practised touch this pressure can be practically confined to the tissues of the abdominal wall without causing any notable pressure upon the abdominal contents.

Search in this way in a fibrositic case will reveal that the pain is brought out sharply and quickly ; it may be localized to one spot (which may or may not be a localization characteristic of visceral disease) ; but it is also very frequently elicited in *several* focalized areas in the abdominal wall. The rectus muscle, for instance, is very commonly affected, and there are often numerous spots of tenderness (with or without thickening) throughout its area. The absence of muscular rigidity in this affection is also specially noteworthy. Sometimes the pain will be marked on pressure over the bony margins of the abdomen, whether ribs, xiphisternum, or pubic crest ; in these situations it is clear that finger-tip pressure cannot be exerted on subjacent viscera.

When a broader pressure by part of the flat hand is employed the pain reaction will be very much less, even with relatively considerable pressure deep in the abdomen. If, on the other hand, the pain is due to visceral disease this is not elicited characteristically by the finger-tip pressure, but is brought out by the broader and stronger pressure more deeply into the abdominal cavity. It is obvious that one needs to be educated in these methods of differential palpation, an education which is largely neglected in regard to this particular method of exploring abdominal pain. Indeed, it is my impression that the fibrositic causes and varieties of abdominal pain are among the most forgotten things in surgical teaching. Perhaps it is because there is the feeling that ninety-nine innocent appendices should suffer rather than that one sinful one should escape surgical execution. However, as a physician I still counsel the academic desirability of accurate diagnosis, even in these days of specialized and mechanical investigation whereby delicate palpation is rapidly in danger of becoming a lost art.

To sum up the diagnostic criteria ; in fibrositic abdominal pain the following tendencies are observed: (1) The painful areas tend to be more or less localized ; they are frequently multiple and are easily and sharply defined by finger-tip pressure, while they are relatively unaffected by flat-hand pressure. (2) They may occupy positions not usually associated with visceral disease ; they are often found on the bony margins where the pain cannot be due to pressure on subjacent diseased viscera. (3) There is great variability in intensity ; the pain may be intermittent, and there is a frequent relation to strain or to weather. (4) Careful investigation of the history fails to reveal a syndrome characteristic of visceral disease, and this in many cases is confirmed by the good general health of the patient. The differential diagnosis might be much further elaborated, of course, but much reliance may be placed on these points provided they are applied with the necessary skill and duly tempered by diagnostic discretion.

LUMBAGO

This is either a localized or a diffuse fibrositis ; in the former it causes those cases of agonizing pain which lock one in a fixed attitude or prevent one from normal locomotion or bending. The recognition of the true pathology will lead logically to those methods of treatment which will almost certainly bring relief, and will eliminate many of those unnecessarily prolonged periods of rest in bed so often prescribed.

SCIATICA

It is true to say that just as fibrositis is the commonest cause of chronic headache so it is of sciatica. The gluteal muscles, as might be supposed, are very often affected by a fibrositis in the septal tissue strands. When this fibrositis spreads to the sciatic nerve as it emerges from the pelvis, a secondary perineuritis results, just as is seen in the case of the supra-orbital nerve. Two signs are usually present: (1) marked and usually maximal pain on deep palpation over the sciatic nerve at its point of emergence ; and (2) pressure pain in other areas of the gluteal region remote from this diagnostic spot.

These two findings almost invariably go together in a fibrositic sciatica. It is, of course, possible to get a gluteal fibrositis without sciatica ; indeed, this is quite common. But very rarely does one find this type of sciatica without a more widespread invasion of the gluteal fibrous tissues ; such a case is theoretically conceivable, and, I suppose, sometimes occurs, though I have never seen one. The practical application of this clinical fact

is that most cases of sciatica call for and yield to massage, always providing that it is skilfully given—a very important proviso.

OTHER SITUATIONS

These are too numerous for detailed analysis ; peri-arthritic cases are common, and it is this type which improves so wonderfully with massage. The pain, particularly in the lower limbs, is so often described as a searing or burning pain ; but careful exploration of the site will result in the characteristic pain on pressure of the periarticular and aponeurotic structures. "Fibrositic heel," which can be a very refractory condition, deserves recognition; a corresponding condition may be met with at the point of the elbow.

Nodular Cases

Much confusion appears to have resulted from the use of the term "nodular," in that it has led observers to expect sharply defined, very obvious, and possibly visible nodules (they sometimes are). As a matter of fact, fibrous tissue when affected in this way always tends to thicken ; but the thickening, as I have already said, may be slight and in such guise—as strands, tracts, or sheaths—as to escape tactual recognition. Then again, fingers are so different, and skilled finger education is not too common, and is certainly receiving less and less emphasis in the teaching of these days. The nodules are occasionally obvious and visible, but such cases are the exception. For those who desire a fuller description I would refer to Dr. Stockman's most valuable book, and also to a paper of my own[2] with a description of a very marked case, the publication of which seemed to have been largely responsible for "great expectations" in regard to these nodules. Occurring as fibro-muscular thickenings, their perception is often subtle, and it may need a vaselining of the surface and much delicacy to appreciate them. They are, however, objective and demonstrable, although still in the minds of many they are only accessible to the finger of faith.

Treatment

First there is the discovery of the causal factor, particularly whether that be direct trauma or its postural or strain variety, or any toxic or infective condition. I have already mentioned some of these, but it is important not to forget dental sepsis, more particularly perhaps the pyorrhoeic cases and abscessed roots. More frequent resort should be had to *x*-ray examination of the teeth rather than to the wholesale clearing of the mouth, comparable to the light-hearted way that "fibrositic" appendicectomies are performed. Preliminary rest is called for in certain types of case, as to which general experience and common sense is a sufficient guide. This is very marked in post-influenzal and some of the gonorrhoeal cases. The juvenile rheumatics also demand rest, because their fibrositic pain is so often coeval with an insidious cardiac damage. On the other hand, rest is not indicated in headache or in many of the chest cases, particularly in the "pseudo-cardiac" examples ; and but seldom in abdominal cases when diagnostic certainty has been arrived at. Many lumbagos and sciaticas also receive a far greater amount of rest than the nature of their case merits.

Drug treatment is only palliative to relieve exacerbations of pain, but is often called for. Outstanding is aspirin, which in many cases is given in far too small doses. While it is a wise procedure to test a patient's reaction to aspirin for the first time the drug is exhibited —though it is seldom that any medical man to-day has this opportunity—it is equally certain that once a patient's tolerance is known there is too much timidity about the dosage. The value, and safety, of a 20-grain dose (repeated in half an hour if necessary, which it very seldom is) has to be known from experience to be appreciated.

Before the war massage and movement as a form of treatment for fibrositis, apart from the purely traumatic varieties connected with the obvious injuries, such as fractures, dislocations, etc., was very little used. The English schools (of massage) originally seemed to avoid any massage that directly touched and exacerbated the painful spot, with the result, naturally, of little or no benefit. In supposed fibrositis, and if the massage does not relieve, either the diagnosis or the technique of treatment is wrong ; and even to-day this most important form of treatment is, in my opinion, not adequately expounded and demonstrated in the English training schools. It is the only curative treatment, and demands much more extensive use than it is even yet receiving. The massage clinics of hospitals are encountering an ever-expanding need in this direction ; for in inveterate and widespread cases effective treatment is tedious, long-continued, and relatively costly. This latter factor is of great importance to the less wealthy middle-class patients, who do not resort to hospitals. Masseurs now abound, and competition reduces charges; but high-grade skill (and experience) in the fibrositic cases is still not too easily come by. Therefore a poor result does not necessarily invalidate the diagnosis.

REFERENCES

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[2] Telling, Maxwell: *Lancet*, 1911, ii.

TUBERCULOSIS IN HOME-CONTACTS

A NOTE ON INCIDENCE AND THE ROLE OF HUMAN CONTAGION *

BY

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Incidence of Tuberculosis in Home-contacts

Investigations of tuberculous infection in home-contacts have been carried out by comparing the frequency of positive tuberculin tests in home-contacts and in non-contacts, or children having no history of exposure to tuberculosis. A critical summary of these investigations has been made by Hart (1932). Among the more important recent workers in this connexion must be mentioned Bernard and Debré (1920), Parisot and Saleur (1925), Opie and McPhedran (1926), Schlesinger and Hart (1930), and Dow and Lloyd (1931). Consideration of their researches leads to the following conclusions.

The incidence of tuberculous infection is much greater in home-contacts than in non-contacts, the younger the child the more marked the difference. The great variation found in the figures of different investigators may be accounted for by several factors: the environment in which the investigation was carried out—for example, town or country, hospital, dispensary, or school ; the care with which controls are chosen as regards excluding contact with tuberculosis in the history and cases of clinical tuberculosis in the non-contacts, and whether children who are attending hospitals and dispensaries or only healthy school children are employed ; the hygienic conditions of the households (Bennighof, 1925 ; Dow and Lloyd, 1931) ; and the number of children investigated, which may be too small.

* An extract from a report, "The Prevention of Tuberculosis in Childhood, with Special Reference to Methods of Separation." Work done during the tenure of a Dorothy Temple Cross Research Fellowship in Tuberculosis, 1933–5, awarded by the Medical Research Council.

TABLE I.—*Reported General and Tuberculosis Mortality among Home-contacts*

Country	Author and Date	Contact with	Degree of Illness of Tuberculous Individual	Age of Home-contacts	Number Investigated	General Mortality	Tuberculosis Mortality
						per cent.	per cent.
France[1]	Armand-Delille (1912)	Household (? parents)	"Tuberculous"	?	175 (Households)		40.0
"	Bernard and Debré (1920)	Mother	"	Infants[2]	58	20.0	? Ditto
"	Calmette (1926)	"	"Tuberculous"; supervised by dispensaries	0 to 1 year	1,364		Provinces: 24.0[3] Paris: 32.6[3]
"	Office National d'Hygiène Sociale, Paris (1928)	"	"Tuberculous"	1 month to 4 years	1,989	24.2	18.0
	[quoted by Lissant Cox et al. (1929)]	Tuberculous environment (excluding above)	"	"	2,865	18.0	13.9
Germany	Braeuning and Neumann (1929)[4]	Mother, father, sibling, or grandparent	"Open" cases; sputum positive, or fatal	0 to 1 year	246	13.0	6.1
						14.5	7.6[5]
				1 to 5 years	569	1.9	1.4
"	Peiser (1929)	Household	"Open" pulmonary tuberculosis	0 to 1 year		5.1	
				1 to 2 years		3.4	
"	Deutsch-Lederer (1929)	"	"Open" pulmonary tuberculosis (sputum positive)	0 to 1 year	196	7.1	6.6
"	Frantz (1930)	"	" "	"	119	22.6	16.8[6]
Denmark	Kjer-Petersen and Ostenfeld (1928)	"	"Open" tuberculosis	"	245	7.7	4.9
		Mother or both parents	" "	"	101		3.9
		Father	" "	"	116		6.9
"	Heckscher (1928)	Mother	Active pulmonary tuberculosis	"	204	24.0	5.4
		"	Quiescent or arrested pulmonary tuberculosis	"	164	16.8	1.8
Holland	Heynsius van den Berg (1929)	House hold	Sputum positive	"			12.3
		"	" "	0 to 2 years			20.4
		"	" "	0 to 1 year	123		7.2
		(already known to dispensary before birth)					
		"	" "	0 to 2 years			12.3
		Mother	" "	0 to 1 year	78		11.1
		"	" "	0 to 2 years	59		20.3
America	Asserson (1927)	"Family"	Tuberculosis	"	149	56.0	47.0
		Mother	"	"	77		35.0
		Father	"		32		62.0
		Other members of family	"		30		56.0
England	Dickinson (1925)	Mother	Tuberculous	0 to 1 year	49	32.6	
		Father	"	"	118	15.2	
"	Cummins (1928)	Family	"	0 to 5 years			Not more than 4.5
"	Lissant Cox et al. (1929)	Household	Sputum positive	0 to 1 year	1,023	8.3	1.7
				1 to 2 years	876	4.7	2.6
				2 to 5 years	501	4.5	1.8
				0 to 5 years			2.7
		"	Sputum negative	0 to 1 year	423	5.6	0.7
				1 to 2 years	368	2.9	0.8
				2 to 5 years	211	2.8	0.9
				0 to 5 years		2.8	1.8
		Mother	Sputum positive	0 to 1 year	244	11.4	3.2
		Father	" "	"	588	7.99	1.3
"	Turner (1930)	Household	Sputum positive, ending fatally	"	76	21.0	5.2
				1 to 2 years	76	13.1	3.9
				2 to 5 years	110	5.4	3.6
"	Dow and Lloyd (1930)	Mother	Sputum positive	0 to 1 year	159	4.4	1.88
				1 to 5 years	105	1.9	0
				0 to 5 years	264	3.4	1.1
		Father	" "	0 to 1 year	169	7.6	1.7
				1 to 5 years	114	6.1	1.7
				0 to 5 years	283	7.0	1.7
		Parent	" "	0 to 1 year	328	6.0	1.8
				1 to 5 years	219	4.1	0.9
				0 to 5 years	547	5.3	1.4
		"	Sputum negative	0 to 1 year	210	5.2	0.4
				1 to 5 years	138	3.6	0.7
				0 to 5 years	348	4.5	0.5
"	Tattersall (1934)	Household	Sputum positive	0 to 1 year	240	11.2	2.9
		"	Sputum negative	"	866	5.4	0.4
		"	Sputum positive	0 to 5 years	223	14.3	5.8
		"	Sputum negative	"	687	14.3	2.3
		Father	Sputum positive	"	111	9.9	4.5
		"	Sputum negative	"	261	13.7	3.8
		Mother	Sputum positive	"	84	21.4	9.5
		"	Sputum negative	"	198	13.1	1.0

[1] In considering general and tuberculosis death statistics in France the following two points should be remembered: (*a*) tuberculosis is not notifiable; (*b*) the difficulties as regards death certification seem to be considerable. Thus Hazemann (1933) states that the causes of death are often "vague and uncertain." In 1929, of 739 0 0 deaths, in 90,000 (or 12 per cent.) the cause was not specified or remained ill-defined ("in six counties one ignores the cause of one-third of the deaths"): in addition, 12 per cent. are put down to "senility" and as many more as "other illnesses"—that is, about one-third of the deaths do not permit of scientific tabulation. He also states that in 1927, while 71,117 deaths were certified as due to tuberculosis, 17 per cent. of the deaths that year were not certified by a medical man.

[2] These include infants who were separated from mother for varying periods.

[3] These figures represent infants dying of affections "presumably tuberculous." Moreover, they are often quoted in France as representing the tuberculous mortality of infants born in tuberculous households.

[4] An extensive and very careful investigation at Stettin. Their original paper should be consulted for many interesting findings.

[5] Period 1906-6 only, when accurate figures obtained with greater certainty.

[6] Correction required for two children who died of tuberculosis at beginning of second year.

TUBERCULOSIS MORTALITY AND GENERAL MORTALITY

In Table I are summarized the figures given by various authors in different countries for the tuberculosis and the general mortality in home-contacts. The degree of contact, the age of the children, and the number investigated are indicated where available. The following conclusions can be formulated.

1. The tuberculosis mortality in home-contacts is much greater than in non-contacts, being high in the first year of life and very much lower afterwards. But the difference is more marked as the child is older, according to some inquiries (Lissant Cox *et al.*, 1929 ; Turner, 1930) ; and less, according to others (Braeuning and Neumann, 1929). Braeuning and Neumann (1929) point out that a large proportion of the tuberculosis deaths in the age group 2 to 5 years is due to children exposed in infancy.

2. The great variation in the figures of tuberculosis mortality reported by different authors may be accounted for by the following factors. (*a*) Conditions peculiar to the country under investigation—for example, the general infantile mortality is higher in France than in England and Wales (83.3 and 70 respectively in 1927). (*b*) The hygienic conditions under which the population studied lives: thus Calmette's figures (1926) were obtained among children living in bad home conditions. (Lissant Cox *et al.*, 1929, when considering separately tuberculous households with good and bad hygiene, found the tuberculosis death rates to be 1.45 per cent. and 1.71 per cent. respectively, and the mortality from all causes 7.05 per cent. and 9.22 per cent.) (*c*) Attention must be paid to the possibility of a number of children having been separated from the tuberculous person in the household for a varying period, however short, as the result of the patient's being away for treatment, or of the child's being sent to stay with relations ; especially as this will probably occur during the period of greatest contagiousness. This factor may partly account for the low figures obtained in Lancashire (Lissant Cox *et al.*, 1929), a county known for its very efficient tuberculosis organization.

3. The figures for mortality from causes other than tuberculosis also differ. The low general mortality figures in home-contacts obtained by Lissant Cox *et al.* (1929), Deutsch-Lederer (1929), and Braeuning and Neumann (1929) are probably due to the fact that (1) infants in tuberculous households are likely to be better looked after as the result of supervision by tuberculosis and child welfare organizations ; and (2) the more debilitated children are more likely to die from tuberculosis. It is also probable that figures, both as regards the tuberculosis and the general mortality, will depend on the proportion of tuberculous mothers and fathers in these investigations, as infants of sick mothers are not likely to receive as much general care in the home. Thus in the Lancashire inquiry the proportion of tuberculous fathers in the households was very high.

EFFECT OF FAMILY RELATIONSHIP

The importance of the relationship of the home-contacts to the tuberculous person in the household is obvious, as a greater incidence of tuberculosis in home-contacts of tuberculous parents may be brought forward as evidence in support of the inheritance of tuberculosis (especially if it is found to be greater when the mother is tuberculous than when the father). On the other hand, as will be seen later, such a fact can easily be explained on the assumption of intimacy of contact increasing the dose of contagion. The work of Schram (1922), Dickinson (1925), Klostermann (1926), Opie and McPhedran (1926), Kjer-Petersen and Ostenfeld (1927), Asserson (1927), Lissant Cox *et al.* (1929), Heynsius van den Berg (1929), and Dow and Lloyd (1930, 1931) indicates that the incidence of both tuberculous infection and mortality is greater when there is contact with a parent than with a more distant relative, with the mother than with the father, this only holding true if no separation is effected (see also Table I).

The value of investigations into the incidence of tuberculous disease in contacts is doubtful, owing to the different criteria adopted by various authors as to what constitutes tuberculous disease. The work of Opie and McPhedran (1926), Chadwick (1927), Barclay (1932), and Armand-Delille (1933) may be mentioned.

As a final reflection in this section, attention should be drawn to the fact that in view of the remarkably wide differences in the tuberculosis morbidity, lethality, and mortality in home-contacts reported by various authors, it is clear that, before any reliable conclusions can be drawn with regard to any prophylactic measure in these children, uniform criteria upon which such investigations are to be based must be determined. It is suggested that this matter could best be dealt with by the Health Organization of the League of Nations.

The Role of Human Contagion

The fact that human contagion can cause tuberculous disease rests on a definite experimental basis. Its role in the tuberculosis of home-contacts can best be considered by discussing the effect of various factors associated with contagion.

It is obvious that in dealing with tuberculosis in home-contacts contact with a tuberculous person is *ipso facto* present. The role of human contagion can, however, best be appreciated by investigating the possibility of contact in children with tuberculous infection and disease not specially brought up because of a history of exposure. The figures obtained by various authors differ considerably (see Bernard and Debré, 1923 ; Lemaire, 1925 ; Debré and Cordey, 1925 ; Bernard, 1927 ; Chown and Medovy, 1933 ; and Bentley, 1933). It seems that the younger the child the more likely is home contagion to be found. This is easily understood, for the infant lives strictly within the family, while the older child spends a great deal of his time out of doors or at school, and may pick up contagion where it cannot be traced. That a history of contact is also more common in children with tuberculous disease than merely with infection is also easily explained by the fact that home-contacts are more liable to receive a large dose of contagion, which is more likely to develop into disease. To a certain extent results will depend on the thoroughness of investigations undertaken in the search for contact ; whether merely the word of relatives is taken or all members of the household are actually examined. They may also depend on the amount of human tuberculosis of bovine origin in the country under review: the more bovine tuberculosis the smaller the percentage of human contact found. The latter factor will not play such an important part in investigations of children with pulmonary tuberculosis (where the bovine type is rather rare*) as in those with surgical tuberculosis or merely a positive tuberculin test.

That contact does not always lead to infection is evident from the fact that not all home-contacts are tuberculin-positive (see Hart, 1932). The following investigations in particular—some demonstrating that children even after prolonged or intimate contact may remain tuberculin-negative—must be mentioned: Köffler (1921), Barchetti (1921), Bernard and Debré (1923), Wahl and Gerstenberger (1923), Petschek (1924), Zimmermann (1925), Myers and Kernkamp (1930), Park, Kereszturi and Mishulow (1933). It is obvious that in these investigations account must be taken of other factors. These will now be discussed.

AGE WHEN CONTACT OCCURS

Cases of tuberculous disease presumably due to postnatal contact have been described at a very early age. Habitz (1927) reports a fatal case at the age of 4 months. Couvelaire and Lacomme's (1931) second case died at the age of 15 weeks. Blacklock (1932), in carrying out

* Recent investigations in Denmark have shown that pulmonary tuberculosis of bovine origin is by no means uncommon (Jensen, 1934).

post-mortem examinations, found tuberculous lesions in six infants under 3 months of age. Bernard and Debré (1923) conclude that the younger the infant at the time of contact the graver the prognosis. Lemaire (1925) found that if contagion arose before 1 year the mortality was 57 per cent.; if between 1 year and 2½ years, 1.66 per cent. Nassau and Zweig (1925) found that of twenty infants infected during the first three months all died; of sixteen infected during the second three months 18.75 per cent. died; and of thirteen infected during the second half of the first year only 3.8 per cent. died. Asserson (1927) states that of those infected under 12 months 15 per cent. die of tuberculous disease, and 2.6 per cent. of those infected between 1 year and 2 years of age.

Braeuning and Neumann (1929) discovered that the tuberculosis mortality varied according to the age at which contact took place (all contacts of open sputum-positive cases).

TABLE II

Age when Contact Began	Number of Cases	Tuberculosis Mortality	
		Within One Year	At end of Five Years of Contact
0-1 year	246	6.1 per cent.	8.1 per cent.
1-5 years	539	1.4 ,,	1.4 ,,
6-10 ,,	618	0.49 ,,	1.4 ,,
11-15 ,,	617	Nil	1.0 ,,

Heynsius van den Berg (1929) observed that when there was contact in the first three months of life 71.8 per cent. of the children became Pirquet-positive before one year, and the tuberculosis morbidity and mortality were 32.1 and 18.3 per cent. respectively. If contact took place between the fourth and twelfth months the corresponding percentages were 52.2, 8.3, and 2.1. At the end of two years the percentages for tuberculous infection, morbidity, and mortality were 80.5, 45.6, and 30.5 if contact had occurred in the first three months of life, and 80.9, 31.5, and 11 respectively if contact first began at a later age. Ribadeau-Dumas (1933) gives the mortality after infection in the first three months as 92 per cent., in the first year as 50 per cent., and in the second year as 10 per cent.

DURATION OF CONTACT

Bernard and Debré (1923) and Lemaire (1927) provide figures illustrating the direct relation between duration of contact and infection. Frantz (1930) shows a similar relation between duration of contact and mortality. The importance of intimacy of contact in relation to infection and mortality has already been indicated (see also Table I), for children clearly have a more intimate contact with the mother than with the father or sibling. The greater incidence of infection and mortality as the result of contact with sputum-positive rather than negative cases, with advanced than with mild disease, is revealed in the investigations of Schram (1922), Röpke (1923), Opie and McPhedran (1926), Braeuning and Neumann (1929), Heynsius van den Berg (1929), Lissant Cox *et al.* (1929), Schlesinger and Hart (1930), and Dow and Lloyd (1930, 1931). Braeuning and Neumann (1929) observe that severity of case and intimacy (good or bad home conditions) are of little importance in the age group 1 to 4 years.

A fact of great importance in relation to the question of heredity is demonstrated by Schlesinger and Hart (1930) when they state that the percentage of tuberculin reactions among children exposed in tuberculous households to parents or siblings with non-pulmonary tuberculosis was no higher than among children of non-tuberculous households.

Bernard and Debré (1923) consider the ante-allergic period (that is, the time elapsing between contact and the appearance of a positive tuberculin test, a term first employed by Debré and Jacquet in 1920) to be inversely proportional to the dose of contagion. Bernard and Debré (1920) estimate that the ante-allergic period may be as long as four months. Epstein (1926, 1928) thinks this is too long, and gives three to seven weeks as the limits when the intradermal test is used. Breckoff (1924) found it to be usually fifty-two to fifty-four days. He quotes Gutowosky (sixty-two days), Schloss (seventy days), and Bratusch-Marrain (fifty-six to eighty-four days). (See also Debré, Paraf, and Dautrebande, 1921.)

PROGNOSIS OF INFECTED INFANTS

Hart (1932) summarizes the published reports on the fate of infants with positive tuberculin reactions. The tuberculin mortality in these varies from 2 to 100 per cent., but of twenty-one authors thirteen report percentages well over 50. Hart concludes that

"it is probable that the divergent results are mainly due to differences in clinical material, although variation in the length of the period of observation may play a part. Much of the work has been carried out in children brought to hospital on account of symptoms, many even being clinically tuberculous when first seen. There is fairly general agreement that even at the present time the prognosis of tuberculous infection in infants is unfavourable once it has progressed to the stage of manifest tuberculosis. . . . Where, on the other hand, the group investigated has consisted mainly or entirely of children in whom the only sign of tuberculosis is a positive reaction, a much lower mortality from this disease has been recorded."

Bernard and Debré (1923) consider the period of survival after the ceasing of contact as of importance in the prognosis. If the infant lives more than six months it escapes progressive tuberculous disease; of thirty-six infected infants who died, thirty died within one month or less after separation from the tuberculous relative, one died after two months, one after four months, and one after five months: that is, if the infant lives for one month after separation there is a good chance of his remaining alive.

SEPARATION OF INFECTED CONTACTS

The question as to what effect has separation from the tuberculous relative of an infant already infected is very important, but difficult to answer.

Bernard and Debré (1923) found at the crèche at Laennec Hospital that forty infants from tuberculous mothers were infected out of a total of fifty-eight, but only thirty infants dated from the period when the crèche was organized so that separation between infants and mothers could be effected. Of these thirty infected infants only twelve died (contact having lasted from two to thirteen months, but in one case thirteen days); the other eighteen (examined two months to two years after separation) remained quite well. They conclude that these eighteen infants were saved because of the separation. This assumption seems hardly justified, as it implies a 100 per cent. mortality for infected infants, which is by no means the rule.

Lemaire (1925), who followed up a hundred children under 2½ years with a positive tuberculin test from three months to over three years, found three deaths (4.5 per cent.) among sixty-seven children who had been separated (in two cases the tuberculosis had already been generalized when separation took place), and twenty-one deaths (63.6 per cent.) among thirty-three children who had not been separated. Separation had been effected forty-eight times by sending the child to the Oeuvre du Placement Familial des Tout-Petits* (P.F.T.P.); once by admission of the parent to

* An organization boarding out infants and children up to the age of 4 with families in the country, under medical supervision.

a sanatorium ; fourteen times by the death of the tuberculous parent ; and once by sending away the nurse. Ribadeau-Dumas (1925), at the same meeting at which the above results were reported, affirmed that too much importance was being attached to the size of the dose or repeated doses of contagion, but that it was the age at which contact took place that mattered: once the tubercle bacillus entered the infant's organism "anything might occur" ; if infection took place in the first three months it was always fatal.

Asserson (1927) mentions that of twenty-three infants from tuberculous mothers referred for boarding out by the New York Tuberculosis and Health Association, only nine whose tuberculin tests were negative were accepted: all were well at the end of a year. Of the fourteen with a positive tuberculin test who were refused, twelve died before the end of the year ; the two who survived had been removed from their homes and placed in institutions for three or four months while their mothers received hospital care.

Bernard, Debré, and Lelong (1925) give the following figures in relation to the children accepted for boarding out by the P.F.T.P. No case of tuberculous disease is stated to have occurred in the 265 infants who had been separated at birth or before contamination had taken place. Among 171 infants and children admitted contaminated (that is, with a positive Pirquet test) there were seventeen cases of tuberculosis (of which thirteen were fatal, giving a tuberculosis mortality of 7.6 per cent.) ; the general mortality is not given. Of sixty-six children who did not satisfy the conditions of admission or whose separation the parents refused, fifty-four (82 per cent.) died, mostly before 1 year, the oldest at the age of 23 months. Their comparison of 82 per cent. and 7.6 per cent. as indicating the benefit of separation of children already infected is not justified, especially as the group represented by the lower percentage are cases selected because of their general good health and after observation so as to exclude tuberculous disease. But in the group of sixty-six children inquiries were made as to contact and hygiene in thirty-seven families. They found that thirteen infants would have been accepted if the parents had not refused. In five contact had ceased owing to the death of the tuberculous parent ; all were alive (in three the hygiene of the environment was satisfactory, in two it was bad). In eight where contact persisted, all died (four environments satisfactory and four bad). Among twenty-four infants whose admission was refused by the P.F.T.P., in eleven contact had ceased and ten were still alive (in six the hygienic environment was good). In thirteen contact persisted and they all died (in ten the hygienic environment was good).

The evidence cited above certainly suggests that, at any rate as regards infants, separation, even after the Pirquet test becomes positive, has a beneficial effect in the prevention of the development of tuberculous disease. It might be argued that this is due not to the avoidance of repeated doses of contagion but to the improved hygienic conditions of the environment in which the infants are placed: these prevent the tuberculous infection from developing into disease. Bernard, Debré, and Lelong's figures relating to infants remaining at home when contact has ceased suggest, however, that repeated doses of contagion do play some part. Any further evidence on this question both from the P.F.T.P. and the Oeuvre Grancher cannot be taken into account owing to the practice of vaccinating the infants with B.C.G. since the time when the children discussed above were dealt with.

Conclusions

1. Post-natal contact is by no means always fatal, and does not always infect even infants, but it causes infection in a large proportion of such children.

2. The younger the infant the more likely is it to be infected, and the younger when infected the graver is the prognosis.

3. While dose (intimacy and duration) plays an important part in infecting an infant it is probable, but by no means certain, that it plays a part in the prognosis afterwards.

4. Separating an infant in contact with a tuberculous parent before infection has occurred prevents the development of tuberculous disease, at any rate up to the age of 4 years (according to the observations in the P.F.T.P.).

5. It is possible that separating an already infected infant may also prevent, in some cases, the development of tuberculous disease, but this may be due to the improved hygienic conditions rather than to the avoidance of repeated doses of contagion.

6. There is no evidence to show whether the separation of an older child with a positive tuberculin test has any effect in preventing the development of tuberculous disease, and it is to be noted that the tuberculosis mortality in children at that age is very low in any case. Hygiene may, however, also have some effect here. Nevertheless, separation is indicated in non-infected children, at any rate up to the age of 5 years, although the danger when contact first takes place in this age period is not nearly as great as in infants (Braeuning and Neumann, 1929).

I should like to express my thanks to Dr. N. Tattersall for permission to publish his mortality figures included in Table I.

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AN INSULIN-RESISTANT DIABETIC

BY

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AND

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We are recording an extreme case of insulin resistance in a diabetic who showed no response to hundreds of units a day. None of the complicating factors which are recognized as interfering with insulin action were present. Such factors have been classified by MacBryde[1] and others as follows: (1) infections; (2) other endocrine disease—hyperthyroidism, pituitary disorders (?), suprarenal disorders; (3) hepatic disorders, including haemochromatosis; (4) pancreatic destructions; (5) cardiac decompensation and extreme circulatory failure. When any of these conditions complicates diabetes it is well recognized that abnormally large doses of insulin are needed to abolish ketosis and control blood sugar.

In the case to be recorded heart disease was present, but it is not considered that this alone should account for the inefficacy of the very large amounts of insulin which were given. Root, in 1929, calculated that a "completely diabetic" subject would require about 200 units of insulin daily. In a few cases more than this is required, but usually one of the factors mentioned above is present, and even then really large doses of insulin produce the usual effect.

There is, however, an extremely small group of insulin-resistant cases in which no explanatory cause can be found, and the present case seems to belong to that group. In our previous experience (R. D. L.), however, *all* cases have responded to insulin when the dose was progressively raised, whereas the present one showed no response.

Case Record

The patient, a slightly obese man aged 63, was in good health until April, 1931, when he developed a coronary thrombosis and came under the care of Dr. W. Maxwell Penny, to whom we are indebted for his history. From this attack the patient made a fairly good recovery. In October, 1932, he became dyspnoeic, and soft systolic cardiac murmurs were present. At this time both (?) pernicious anaemia and diabetes were diagnosed (red blood cells, 2,160,000 per c.mm.; haemoglobin, 35 per cent.). The blood count rose to 4.6 million with hepatex, and the diabetes required 60 to 90 units a day for its control for some weeks only, while ascites and pleural effusions were present. Later the glycosuria was controlled by diet alone.

Apart from a return of dyspnoea early in 1934, which responded to treatment, the patient remained fairly well until October 19th, 1934, when severe glycosuria and ketonuria were found to have recurred after a "cold" and remained uncontrolled by diet. A week later insulin was again started, and was increased to 40 units three times daily without improvement of the diabetic condition.

On November 6th the patient was admitted to King's College Hospital. The temperature was subnormal, the pulse feeble (110 per minute), and the blood pressure 105/75. The heart was enlarged, but the apex beat could not be defined as marked pulmonary emphysema was present. Evidence of cardiac failure—such as basal or generalized oedema, enlarged liver, or venous congestion—was entirely absent. The knee- and ankle-jerks were absent, but no other neurological abnormalities were found.

The urine contained 8.2 per cent. of sugar, large amounts of ketone bodies (strongly positive $FeCl_3$ test), and a trace of albumin. The blood sugar on admission—six hours after 100 units of insulin—was 0.354 per cent., but clinical signs of impending coma were absent and remained absent throughout the illness. Carbohydrate (30 grams) and insulin (50 units) were given four-hourly without any reduction of the ketonuria and glycosuria, neither of which showed any improvement until the patient's death six days later; nor was there any significant change in the blood sugar. The carbohydrate was finally increased to 60 grams four-hourly and insulin to 160 units four-hourly as shown in the table. Some of the insulin was given intravenously, and there was no reason to think that the subcutaneous injections were not absorbed.

Day after Admission	Diet: Total Carbohydrate	Total Insulin Units	Minimum Blood Sugar	Urine Sugar Average	Ketonuria: Fe Cl3 Test	Blood Pressure and Pulse
			Per cent	Per cent.		
1	120 grams	190	0.354	8	++	105/75; pulse, 110
2	150 ,,	320	0.294	7	++	124/70; pulse, 80
3	180 ,,	560	0.310	8	++	103/75; pulse, 90
4	Glucose 300 grams	720	0.370	10	++	112–158/80–95 pulse, 92
5	Laevulose 360 grams	960	0.403	12	++	130/90; pulse, 135

The cardiac condition improved temporarily under the influence of digitalis and alcohol, the blood pressure rising at its highest to 150/92 and varying considerably (see table). Shortly before death marked pulsus alternans was present, and the pulse rate rose to 136. The patient never became comatose, air-hunger and vomiting were absent, and he was never obviously dehydrated, though a blood count a few hours before death revealed: red blood cells, 6,220,000 per c.mm.; haemoglobin, 118 per cent.; and a leucocytosis of 14,400 per c.mm. with 84.8 polymorphs, though no infection was present, suggesting dehydration.

A sample of blood taken three days before death showed the following: alkali reserve, 45 vols. per cent.; blood urea, 49 mg. per 100 c.cm.; blood uric acid, 3.5 mg. per 100 c.cm.; serum protein, 6.1 per cent.; chlorides (plasma), 500 mg. per cent.

The urine always contained varying amounts of sugar—between 5 and 12 per cent. The albuminuria increased as the illness proceeded, but was never heavy. The volume of urine remained high (average, 2,000 to 3,000 c.cm.) until death. No casts and no other chemical abnormalities were present, and the chloride content was only slightly reduced.

Post-mortem Findings

(*Abridged from Report by Dr. A. Gilpin*)

Lungs.—Slight bronchitis and non-inflammatory oedema; no pneumonia.

Heart and Blood Vessels.—Moderate generalized atheroma and old thrombosis of anterior descending branch of left coronary artery, with thin heart muscle in infarcted area; right auricle dilated; compensatory hypertrophy of left auricle and ventricle. Muscle fibres in healthier part of ventricle showed brown atrophy and poor striation.

Liver.—Normal weight, terminal congestion; slight fatty degeneration, mostly portal. Gall-bladder, not infected, contained four soft stones.

Kidneys.—Pale, congested, senile atrophy: glomeruli all normal microscopically.

Bone Marrow.—Showed predominantly normoblastic activity; a few megaloblasts seen.

Endocrine Glands.—Entirely normal macroscopically and microscopically. Islets of Langerhans seemed normally abundant, and showed no inflammation or degeneration. Pituitary showed normal proportion of basophil and eosinophil cells.

Discussion

There was no evidence of any insulin action as judged by the unchanging hyperglycaemia, glycosuria, and, more important still, ketosis. The ketosis showed that abundantly available glucose was not being oxidized by enormous doses of insulin, which is unique in our experience, even in almost moribund diabetics. In view of the urgency of the case it was hardly permissible to study the effect of insulin alone without covering carbohydrate, and a fall in blood sugar would almost certainly have

[1] MacBryde: *Arch. Int. Med.*, 1933, lii, 952.

occurred with such a heavy excretion of sugar *per se*, whether the insulin acted or not. The essential criterion, the unvarying ketosis, showed that no appreciable insulin action took place.

The clinical condition and the post-mortem findings indicated no important abnormality, except the cardiac condition. To this, therefore, the failure in insulin action might be ascribed. But the pulse pressure, the absence of oedema, hepatomegaly, and venous congestion, and the high volume of urine showed that a reasonably adequate circulation was being maintained until just before death. We feel, therefore, that the heart condition does not explain the failure of insulin action, and the case seems worth recording.

Summary

A case of diabetes is recorded which failed to respond to hundreds of units of insulin a day.

SOME EFFECTS OF ARTIFICIAL HEAT ON THE CIRCULATION IN COLD TEMPERATE CLIMATES*

BY

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From observations on the effects of cold and heat in chronic rheumatism it seems clear that in a warm-blooded animal a small permanent deficiency of heat may be a factor in the production of disease. The delicate mechanisms of heat production and loss which protect the body against a deficiency of heat may break down. The production of heat cannot always keep pace with its loss, and exposure to cold is followed by a fall in body temperature, first on the surface and afterwards in deeper parts. The thermostable animal has become thermolabile. At the other end of the scale it seems equally clear that upon exposure to extreme heat in the Tropics, and in some fevers, heat loss cannot keep pace with heat production, and that there is an accumulation of heat in the body which may lead to fatal results.

Here are two categories of disease arising from uncompensated exposure to cold or heat. They show a break in thermo-regulation in one direction or in the other. Micro-organisms may also be concerned in these diseases, but chilling or overheating plays an essential part in their causation. The climate of the northern temperate region has for its chief characteristic an incessant conflict of polar and Atlantic currents, and consequent abrupt changes in temperature. This is, therefore, the region in which the effects of sudden changes of temperature should be looked for in sensitive subjects, and especially the effects of repeated and chronic chill.

One lesson which may be learned from the study of rheumatism is that the effects of cold are shown very quickly, and perhaps first of all, in the circulation. Subnormal temperatures are common in rheumatic limbs. It seems that the capillary bed is greatly reduced, and that an abnormal number of capillaries in the cutaneous network are closed. It is known that rheumatism is frequent in exposed parts ; rheumatic joints are very near the surface of the body, and the articular cartilages (loci minoris resistentiae) are ill supplied with blood. This has led to the hypothesis that a large and prevalent type of rheumatic disease is connected with deficiency of blood supply, the result of external cold. Certainly the beneficial effects of protection against chill and the removal of pain, stiffness, and swelling by heat give support to this hypothesis.

The gradual effects of cold on the circulation in the skin and extremities need no description, nor yet the immediate effect of, say, a brief hot bath (three minutes at 100° to 105°). The skin, which was the coldest organ of the body, becomes the hottest ; it changes in colour from a dull white to bright red. The pulse is accelerated, soft, and large, the muscles relaxed, and perspiration breaks out. The observation of this sequence of events is particularly instructive in men who have been subjected to hard labour and exposure. Chill, fatigue, pain, and stiffness give place to a sense of warmth, comfort, and vigour.

Degree and Duration of Heat

The cardinal twofold reaction to heat, not unlike the reaction to many drugs, is shown in the restorative effect of brief baths and the injurious after-effects (the " thermal debility ") of prolonged heat. The medical importance of these physiological facts and their bearing on the treatment of many chronic disorders are insufficiently realized. But even rough-and-ready rules are of some service. Baths over blood heat, prolonged for more than ten minutes, are always injurious in health. In cases of low blood pressure or myocardial weakness only a few seconds' immersion in a bath at blood heat can be allowed. There are great individual differences in the twofold reaction both to heat and to cold. Fairly robust persons may, by training, greatly increase their power of accommodation. Even when it has been considerably diminished, as in chronic disease, it may often be re-educated by judicious thermal applications.

Those who have prescribed medical baths for many years must realize the governing importance of the dosage of heat. Sometimes it must be very precisely measured. In effervescent baths for circulatory cases a single degree may make the difference between a stimulant or a sedative effect. In addition to temperature, there is always the " fine adjustment of duration." Where the patient is progressing well a minute may often be added every two or three days. In each case the duration of the bath must be regulated by the circulatory reaction which it evokes. The disregard of dosage in hot baths in the last century caused a strong aversion to their use, and as a result baths of all kinds were forbidden in heart cases. Similar mistakes were made with baths of low temperatures. Even iced baths were given in typhoid fever, in ignorance of the fact that in febrile diseases external cold below 65° is a dangerous depressant. Such a use of powerful agents, which are really invaluable stimulants or sedatives, did much to bring hydrotherapy into disrepute. [But if no guidance had been given in the use of morphine and strychnine they would certainly have also been banned.]

To their credit practitioners at spas have carefully studied the administration of baths. Good effects often follow their precise use in circulatory disorders—for example, in high blood pressure and hyperpiesia, as described in Dr. Edgecombe's paper presented at the annual meeting of the British Medical Association at Eastbourne.[1]

Consensual Reaction to Heat

Popular medicine everywhere relies a great deal on the local use of heat carried by hot bottles, bricks, flat-irons, poultices, fomentations, steam, warming-pans, foot-baths, *et hoc genus omne*. Everyone knows that hyperaemia induced by external heat is very soothing in a great variety of painful conditions. The rationale of such methods is now better understood In addition to their local effects—relief of pain and circulatory tension, dilatation of capillaries and increased circulation—it is now

* Read before the South Essex Division of the British Medical Association, February 12th, 1935.

[1] *British Medical Journal*, 1931, ii, 319.

known that the effect of heat cannot be confined to the part where it is applied. It has been many times demonstrated by the plethysmograph that if one limb, an arm or leg, be heated in a bath the other members and the entire surface of the body are affected in the same manner. The old maxim that if one member suffers all the others suffer with it has found a new expression in what is known as the "consensual reaction" to heat. Local heat may unpleasantly excite the heart and circulation. Very hot packs of fango, peat, or estuary muds are commonly used in chronic arthritis to improve the local circulation and assist in the resolution of deposits. But these applications frequently disturb the heart, and, to avoid circulatory excitement and subsequent fatigue, it is necessary during the pack to allow iced water to flow through a Leiter's coil over the heart region.

The most characteristic and desirable effect of local heat on the general circulation is the relief of vascular and nervous tension. Consequently at many of the spas partial baths have for many years formed an important part of the cure. The most interesting baths of this kind are foot and arm baths at rising temperatures. These have been employed in conjunction with other physical methods in many spastic troubles, such as asthma, and in chronic respiratory disorders, angina pectoris, and arterial hypertension.

A CASE OF MEGALENCEPHALY SHOWING AN UNUSUAL TALENT FOR CALCULATING DATES

BY

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From time to time there appear in the literature reports of mentally defective children or adults who, against a general background of amentia, are yet able to display special talents in one or more directions.

In the case to be recorded there is an astonishing aptitude for dates. The patient remembers the date of death of various occupants of his ward, the arrival or transfer of doctors and nurses, and the dates of the examinations of his blood and cerebro-spinal fluid. He knows all the patients in his ward by their correct names and can tell the number of patients from day to day. The daily happenings are remembered in detail, also the names, date of entry, and promotions of the nursing staff. He readily recalls the dates of visits of different members of the medical staff. His chief ability, however, lies in the correct naming of the day of the week on which any given date falls.

Various degrees of hypermnesia are not particularly rare among the feeble-minded. Memory in this patient plays a more important part than appears at first sight, for there are grounds for believing that he has committed to memory the day of the week corresponding with some key-date. When given a date at random, he counts backwards and forwards to the nearest one corresponding with his initial key-date, making allowances for a leap-year. It has been observed that he is quicker at calculating future than past dates. Examples of his faculty in this matter can be seen from the appended table of questions which were put to him.

The mental imagery employed by this patient is thought to be of the auditive type. While making a calculation he displays constant rhythmical hyperkinetic movements, at the same time repeating the question aloud. But if the trunk is fixed and he is not allowed to repeat questions, he reckons less quickly and makes errors. There is marked poverty of ideation, which, in the mental defective's mind, tends towards an astonishing concentration of effort of his memory processes.

Family and Previous History

The patient was certified under Section 24 of the Lunacy Act on May 24th, 1915, and admitted to Leavesden Mental Hospital on January 17th, 1919. He was the fourth child in a family of six, and at the time of his birth his mother was 34 and his father 32. The unusual size of the child's head was first noted by his mother when he was 5 weeks old. The patient remembers, however, that his head was still increasing in size at the age of 6 years, and other children used to call him "big-head."

Since July 18th, 1933, the exhibition of luminal therapy has had a marked effect on the reduction of epileptic fits, and he now averages only three or four fits each month. Before epileptic fits he becomes abusive, obscene, and excitable; often refuses food, and is destructive and impulsive.

Present Condition

The patient is an extremely obese adult male, aged 32, whose height is 5 ft. 4 in., and weight 15 st. 2 lb. His head is enlarged in all diameters, ovoid in shape, and brachycephalic with an increase in the transverse diameter. The height of the forehead is exaggerated, but it does not bulge in the way that is ordinarily seen in the hydrocephalic. The maximum circumference of the head, 27¾ inches, is obtained at the superciliary margins; in this respect he differs from the ordinary hydrocephalic, whose maximum cranial circumference occurs in the temporal region. The patient's measurements are: length, 20.5 cm.; breadth, 19.7 cm.; circumference, 70.6 cm.; height, 190 mm.; cephalic index, 81; cubic content, 2,583 c.cm. by Lee's formula. *X*-ray examination of the cranium revealed nothing abnormal.

The patient is remarkable in that not only is the cranium enlarged, but the face also. The condition is therefore suggestive of a true megalencephaly rather than a hydrocephaly. There is marked growth of fine hair on the skull, which is tightly stretched, and the skin covering the face is coarse and pitted. In the neighbourhood of the nose are several small adenomatous masses. Percussion of the cranium yields a box-note, not quite the stony hardness associated with hydrocephaly. The palate is high, with a maxillary prognathism and marked spacing of the upper incisors. The face is symmetrical, and almost as broad as it is long; the nasolabial folds are poorly defined, and the expression is one of facile content. The hands are fat and dimpled, with slight saddling of the digits, the nails unduly curved from side to side, and slightly ribbed, and the fingers tapered. When the patient is stripped it is seen that he is enormously obese, the fat being evenly distributed over the trunk and limbs; there is marked generalized seborrhoea. The lower limbs are hairy, thickened, and indurated. The patient remains all day in a wheeled chair, and although he is unable to walk he can move himself from one chair to another.

Central Nervous System

The eyes show alternating strabismus and nystagmoid jerkings on looking to the right. The pupils are eccentric, equal, and irregular in outline; the fundi are normal. The right pupil is 3.5 mm. in diameter, and the left is 3.5 mm. Both pupils react sluggishly to light and convergence. The cranial nerves and articulation are normal. Dynamometer reading in right hand is 70; left hand 25.

There is a coarse tremor limited to the right upper limb, and the left foot is in a position of talipes varus. All tendon reflexes are exaggerated, more so on the left side; bilateral ankle-clonus; left plantar, extensor; right plantar, flexor. There is no loss of sphincter control. He has hemiplegia affecting the left upper and lower limbs. The left upper limb, which is the same size as its fellow, is adducted, semi-flexed at the elbow. The thumb is adducted and markedly flexed, and the fingers held in a position of hyperextension at the metacarpo-phalangeal joints and semi-flexion at the interphalangeal joints. The muscles of the left forearm and left calf are somewhat smaller than on the right side. Associated movements are present. Spinal curvature exists, with convexity to the right. The circumference of the neck is 18¼ inches. There is no loss of kinaesthetic sensibility. Cerebro-spinal fluid pressure: head flexed, 21.5 cm., 22 cm.; extended, 16 cm., 17 cm. Blood pressure is 170/80. Urinary excretion 50 oz.; examination of the urine shows slight reduction of Benedict's solution.

Sugar Tolerance and Blood Count

There is marked decrease in sugar tolerance; the blood sugar increases up to 221, and at the end of two hours has not returned to the resting figure: 0 hours, 105 mg. per 100 c.cm.; 1/2 hour, 221 mg.; 1 hour, 201 mg.; 1½ hours, 161 mg.; 2 hours, 146 mg. With the obesity, these findings strongly suggest some pituitary disorder. The patient's weight has increased from 8 st. in 1925 to 15 st. 2 lb. in 1934.

Total differential blood count: red blood cells, 5,185,000 per c.mm.; haemoglobin, 113 per cent.; colour index, 1.1; white blood cells, 9,450 per c.mm.: polymorphs 5,220 (55 per cent.), lymphocytes 3,680 (39 per cent.), eosinophils 281 (3 per cent.), monocytes 281 (3 per cent.).

Mental Tests and Mental Characteristics

Burt's revision of Binet-Simon scale: mental age of 5 years 11 months. Burt's test of arithmetic, graded oral test, mental test: mental age of 5.6 years.

Performance Tests of Intelligence

	Score	Mental age.		Score	Mental age
Porteus maze	5 and 4	4 yrs.	Healy picture completion test ...	0	0
Goodenough's drawing test ...	6	4 ,,	Cube construction test	0	0
Cube imitation test	0	0 ,,	Dearborn formboard	0	0
Manikin test	0	0 ,,	Healy construction test "A"	0	0
Goddard formboard	0	0 ,,			
Woodworth and Wells substitution test	0	0 ,,			
Triangle test	0	0 ,,			

The patient can neither read nor write, but readily repeats the alphabet forwards and backwards; he is also unable to tell the time by the clock. He never initiates a conversation, and has no friends among the other patients. He displays a keen sense of humour and appreciates music, but expresses no desires or ambitions, and makes no plans for the future. Moreover, he makes no attempt to occupy himself, having no desire or aptitude for handicrafts. He can always tell when he is going to have a fit, as he has a nasty feeling in the throat and then he begins to shake all over. Before the fit he becomes very abusive and obscene to the nursing staff.

Special Arithmetical and Date Tests

Question	Patient's Reply	Correct Answer
What day is:		
Christmas Day in 1940?	Wednesday	Wednesday
March 3rd in 1940?	Sunday	Sunday
Christmas Day in 1950?	Monday	Monday
May 16th in 1950?	Tuesday	Tuesday
Christmas Day in 1955?	Sunday	Sunday
August 15th in 1935?	Monday	Monday
Christmas Day in 1938?	Sunday	Sunday
Christmas Day in 1950?	Sunday	Sunday
What day was:		
Christmas Day in 1900?	Tuesday	Tuesday
July 3rd in 1905?	Monday	Monday
August 4th in 1914?	Tuesday	Tuesday
What day will Christmas Day be in 1930?	Tuesday	Tuesday
What date will:		
Good Friday be in 1936?	April 17th	April 10th
August Monday be in 1935?	August 3rd	August 3rd
Whit Monday be in 1936?	June 1st	June 1st
The first Monday in August, 1936, is the 3rd; what are the dates of the following Mondays in the month?	10th 17th 24th 31st	10th 17th 24th 31st
What are 9 times 5?	31	45
What are 5 times 9?	52	45
How many 6's in 12?	3	2
How many do:		
9 and 7 make?	16	16
10 and 8 ,,	18	18
6 and 4 ,,	10	10
12 and 11 ,,	23	23
15 and 16 ,,	30	31
13 and 14 ,,	29	27

Questions and Patient's Reaction Time

Question	Patient's Reply	Reaction Time
What day was:		
April 1st, 1893?	Thursday	7 seconds
June 21st, 1852?	Sunday	25 ,,
September 10th, 1804	Friday	15 ,,
June 1st, 1793?	Wednesday	20 ,,
October 27th, 1754?	Monday	35 ,,
December 11th, 1702?	Sunday	40 ,,
December 12th, 1900?	Wednesday	35 ,,
June 7th, 1900?	Thursday	10 ,,
January 1st, 1825?	Saturday	8 ,,
What day is:		
August 21st, 1965?	Sunday	25 ,,
May 10th, 2024?	Sunday	20 ,,
June 10th, 2050?	Tuesday	15 ,,
September 16th, 1999?	Thursday	7 ,,
March 4th, 1950?	Saturday	13 ,,
May 6th, 1971?	Thursday	21 ,,
The day of week for:		
January 1st, 1920?	Thursday	3 ,,
,, 1st, 1917?	Monday	4 ,,
,, 1st, 1923?	Monday	2 ,,
,, 1st, 1914?	Thursday	2 ,,
,, 1st, 1926?	Friday	2 ,,
,, 1st, 1911?	Sunday	6 ,,
,, 1st, 1929?	Tuesday	2 ,,
,, 1st, 1908?	Wednesday	2½ ,,
,, 1st, 1932?	Friday	1 second
,, 1st, 1927?	Saturday	1 ,,
,, 23rd, 1911?	Monday	11 seconds

Questioned as to leap-year day in certain years the patient replied as shown in the following table.

Year	Patient's Reply	Correct Answer	Reaction Time	Year	Patient's Reply	Correct Answer	Reaction Time
1936	Saturday	Saturday	17 secs.	1824	Saturday	Sunday	16 secs.
1932	Monday	Monday	7 ,,	1828	Thursday	Friday	10 ,,
1928	Wednesday	Wednesday	9 ,,	1832	Tuesday	Wednesday	5 ,,
1924	Friday	Friday	7 ,,	1840	Thursday	Saturday	11 ,,
1920	Sunday	Sunday	10 ,,	1848	Monday	Tuesday	6 ,,
1916	Tuesday	Tuesday	4 ,,	1860	Tuesday	Wednesday	10 ,,
1912	Thursday	Thursday	3 ,,	1836	Sunday	Saturday	9 ,,

Remarks

This patient is extremely interested in and proud of his achievement. He displays a remarkable singleness of purpose, and while making a calculation appears to be entirely oblivious of environmental stimuli. Whatever may be the causes producing defective development of the nervous system, it would appear that their incidence on all parts of that system need not be the same. It is possible, consequently, to have one portion of the nervous system proceeding to attain maturity, while the other portions lag behind to a greater or lesser extent. When this happens we get ability in some particular field standing out conspicuously against a background of amentia. The contrast is sometimes so striking as to give the impression of phenomenal capacity as regards the department of knowledge concerned, but this appearance is probably misleading. In their own special way the "idiots savants," as persons exhibiting the features under consideration, are not superior to persons whose general mental level is normal.

I wish to thank the medical superintendent, Dr. R. M. Stewart, for permission to publish this case.

Clinical Memoranda

Epidemic Catarrhal Jaundice amongst Children

In October, 1934, I came across one or two cases of jaundice in very young school children which preceded a mild epidemic of similar cases amongst other children of the same school. All the patients had the same symptoms and the disease ran the same course. Lack of facilities prevented full investigation, but there was a similarity between the symptoms and course to that of a very mild type of Weil's disease (spirochaetosis icterohaemorrhagica). Practically all the cases were in the same age group—5 to 7 years—and all attended the same school, although their homes were scattered about. Running concurrently with the "epidemic" was an epidemic of diphtheria.

SYMPTOMATOLOGY

There were four stages of the clinical symptoms.

1. The mother would notice that the child was listless and inclined to lie about the house. Appetite was poor, and occasionally headache was complained of. This stage lasted about four or five days.

2. At the end of this period the child would appear to be shivering, and the temperature at this stage varied from 99° to 104° F., while the pulse rose to 80 to 100. All the cases now showed one symptom in common—namely, sore throat—the tonsils being swollen, and in most of the cases there was a little "membrane," greyish-yellow in colour, as well as fetor. The concurrent diphtheria epidemic naturally led one to send swabs, which resulted in all cases being reported as "diphtheria-negative." Vomiting was present in most cases. At this stage appeared the first sign of incipient jaundice, a yellowish tinge being found in the conjunctivae. The urine up to this time would not give a bile reaction to nitric acid on filter paper washed in it, but within forty-eight hours the greenish-brown tinge of bile was visible, and with its appearance the temperature would fall practically to normal. The urine was alkaline, and there was some albumin.

3. The second stage lasted about forty-eight hours, and the patient would both look and feel better, but the stools were now light in colour, and in about twenty-four hours were definitely clay-coloured. The skin of the entire body was lightly jaundiced, although itching was not found. The throat symptoms disappeared; only in one case was there a complaint of seeing yellow.

4. The stage of jaundice persisted for five to seven days, the urine first becoming normal in colour, followed by the stools, the latter becoming brown in from seven to ten days. Recovery was complete in another fortnight. As exceptions to the above sequence of events, the spleen was definitely tender in three cases, and in one case there was marked and offensive diarrhoea.

Treatment was on simple lines. Calomel 1/4 grain four-hourly until 1½ grains had been taken, was followed by a saline purge. In two cases where jaundice persisted for two weeks, hexamine, with acid potassium tartrate, was given. Diet was restricted to fluids.

SUMMARY

There were twenty-five cases in all, and each attended the same school; most of these were in the very youngest classes. All had one premonitory symptom in common—sore throat with fauces simulating diphtheria. In connexion with the latter there was a short respite from fresh cases during the Christmas holidays, and in this period I did not come across any fresh cases of jaundice. With the reopening of the school after the holiday the cases of jaundice reappeared with new cases of diphtheria. Another factor in common was that these children all received a daily ration of milk (at school), which they drank through a straw from a bottle, and that they were mostly the children of mine workers.

An account of an outbreak of a similar nature, which occurred in September, 1933, was reported in the *British Medical Journal* (1934, i, 330) by Dr. Montford of Castle Donington, a village about fifteen miles from this one. Since the end of January I have not met with any fresh cases.

Newhall, Burton-on-Trent. E. M. R. FRAZER, M.D.

Subcutaneous Emphysema following Subcricoid Injection of Lipiodol

Subcutaneous emphysema is not a well-recognized complication of injection through the cricothyroid membrane or in the subcricoid region. Only a brief reference is made to the possibility of this accident by Siccard and Forestier in their book *The Use of Lipiodol in Diagnosis and Treatment*, and no indication is given of the frequency of its occurrence. The following case may thus be of interest.

A boy, aged 5, was given 20 c.cm. of lipiodol for suspected bronchiectasis following pertussis. The injection was made without difficulty in the subcricoid region by means of a 20 c.cm. syringe and an ordinary intravenous needle, under general anaesthesia. Within a few minutes of injection a swelling was noticed in the neck at the site of injection. The typical crepitus of surgical emphysema was felt in the swelling, which rapidly increased in all directions. Within about six hours it had reached its maximum, having spread over the thorax and abdomen, down both arms, and over the face (causing considerable distortion of the features) and scalp. No embarrassment was exhibited by the patient, other than that due to the anaesthetic, and after a week most of the air had been absorbed.

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Reviews

RESPIRATION

A second edition of J. S. HALDANE's work on *Respiration* had long been hoped for. It has now eventuated, with the collaboration of J. G. PRIESTLEY,[1] and we note with pleasure the thoroughness with which the new edition has been prepared. In the hands of Professor Haldane, and now in their joint hands, the subject is treated in the widest manner, and with a broad biological outlook. The phenomena described, though arbitrarily separated for purposes of close examination, are always to them something more than a mere interpretation of physical and chemical phenomena, but form part of an organic whole, characteristic indeed of life itself. It is treated, in fact, as a part of a wide science of biology, embracing, but greater than, the physical sciences, since "its fundamental axiom is that the life of an organism, including its relation to environment, implies the co-ordinated maintenance and reproduction which are everywhere in actual fact characteristic of life."

Most of the investigations dealt with have been carried out on the human being, and much of the matter has formed the subject of personal investigations by the authors, particularly the senior one. Perusal of the work and comparison with the first edition shows how much we owe to these investigations carried out in the Oxford school. It is perhaps not too much to say that this and other works of that school have, more than any other publications, been responsible for the now fashionable form of laboratory instruction in physiology, carried out by experiments on man. For this return from the doctrinaire instruction in muscle-nerve physiology to a more immediately biological outlook we have good reason to be grateful. There is also abundant room for praise for the valuable practical results which these researches have achieved, in the realm of medicine, in their application to mining, mountaineering, aeronautics, and caisson work. Our knowledge of breathlessness, cyanosis, carbon monoxide poisoning, has undergone remarkable advances in the last few years, and a full account of the details of all these investigations is given. Most of all, however, we are indebted to Professor Haldane and Dr. Priestley for an enlightening treatise on a still developing branch of experimental science, and a philosophic outlook on a branch of biological knowledge which is of the greatest importance. The index and the valuable bibliography are noteworthy improvements on the previous edition, which lacked both.

INDUSTRIAL MEDICINE

One useful purpose served by the International Labour Office has been the periodical publication during the last twenty-four years of papers of varying length and value upon occupational diseases. These have been gathered and bound together in two volumes, and are issued from the office in Geneva. Volume i appeared in 1913: it is with volume ii[2] that we are concerned. The editorship is centred in Geneva, where, with the aid of polyglot experts, the papers have been sifted, translated, and arranged for publication.

Volume ii begins with ice-cream makers and ends with zinc. Since many trades are dealt with, only a few occupations can be discussed here. As might be expected, considerable space is assigned to the subject of industrial health propaganda, workshops, their interior and equipment. This Dr. Réne Sand deals with in an effective and interesting manner. The article on the lighting of factories is from the pen of the late Dr. Leon Gaster, who never lost an opportunity of impressing upon his audiences the relationship between the number of accidents in factories and defective illumination. During winter in the United States, when greater use is made of artificial lighting, the average number of accidents per hour was found to be higher than by daylight. More accidents occurred, too, at night than in daytime, partly from the greater incidence of fatigue, but probably also owing to insufficient lighting. Out of 91,000 industrial accidents 24 per cent. were attributed to inadequate lighting, a number which closely corresponds with the 23.8 per cent. of certain insurance offices. Experiments have shown to employers the advantage it is to have their factories well lit, by the circumstance that with good illumination the output increased by 20 per cent. In the scientific organization of industry, physiology plays an important part by ensuring that human energy is made use of to the best advantage of society and with the least harm to the individual. Industrial physiology is concerned with more than the mere execution of work. Environment, the personality of the workers, and social factors of various kinds have all to be considered. Many recent researches have reference to physical labour, and therefore deal with muscular activities and energizing foods, with their hydrogen and carbon contents, part of the energy being used for the performance of work, the remainder for the maintenance of the body temperature. The carefully prepared article dealing with these matters, by Professor E. Atzler of Berlin, should be of great assistance to all who are interested in the study of metabolism and work.

The iron and steel trades are fully discussed, and their effects upon the workmen detailed. Mention is made of the hard and fatiguing character of the work, the dusty atmosphere in which it is carried on and the exposure to high temperatures, the risk of accidents and severe burns and poisoning by carbon monoxide, and the high incidence of pneumonia in the men. To the article on metallic lead twenty-one pages are devoted. Dr. G. Frey of Berlin deals in a masterly manner with the metal from its source in the mine, its smelting, and conversion into commercial products, while Professor C. Biondi of Siena, in seventeen pages, has condensed in an attractive manner most that is known of lead poisoning. The symptomatology of plumbism is reviewed and the social effects of the malady considered. While not accepting the opinion that women are more susceptible than men to lead poisoning, Biondi remarks that in women plumbism assumes more serious forms than in men: the functions of maternity are profoundly disturbed, and the children born are undersized and apt to degenerate. To the manufacture of lucifer matches several pages are given, but since the publication of the Thorpe-Oliver report upon this subject, and the substitution of sesquisulphide of phosphorus for the dangerous metalloid in the heads of "strike anywhere" matches, there are no longer cases of industrial phosphorus poisoning. The manufacture of the "safety" or "strike only on the box" match made from red phosphorus has not, until recently, been a cause of illness in the workers. In some factories chrome salts to the extent of 3 to 6 per cent. have been added to the paste used for heading the matches, with the result that manipulation of the material and inhalation of the dust have produced ulceration of the skin and perforation of the nasal septum.

To Dr. Wade Wright of New York has been entrusted the article on "Medical and Allied Professions." It

[1] *Respiration.* By J. S. Haldane, C.H., M.D., F.R.S., and J. G. Priestley, *M.C.*, D.M. Second edition. London: H. Milford, Oxford University Press. 1935. (Pp. 493; 124 figures. 30s. net.)

[2] *Occupation and Health. Encyclopaedia of Hygiene, Pathology, and Social Welfare.* Vol. ii, I-Z. Geneva: League of Nations, International Labour Office. 1934. (Pp. 1310. Paper cover, £4 (2 vols.); cloth, £4 7s. 6d. (2 vols.))

shows the liability of doctors to certain diseases and to infection through minute abrasions of the skin ; also the increasing prevalence among them of heart affections, especially after middle age. Ogle, several years ago, drew attention to the low mortality from pulmonary tuberculosis in medical men ; notwithstanding their employment in hospitals, clinics, and sanatoria, the death rate among physicians from this infection is remarkably low.

The article on coal-mining and its dangers is long, but none too long considering the importance of the industry. Dr. Frey has dealt very thoroughly with the subject, and his article will well repay perusal. To the section on occupational diseases 148 pages have been assigned ; all the papers are remarkably good, Dr. F. L. Hoffman's particularly so. With regard to the volume as a whole we could wish that it contained more papers by British authorities, remembering that it was Great Britain which mainly led the way in industrial hygiene in the early part of last century, and set the pace of legislation which other countries have followed.

OBJECTIVE CRIMINOLOGY

By definition a criminal is one who offends against the penal law of his community, and the penal law of most civilized communities embraces offences varying as widely in quality as murder and blackmail on the one hand, and the unauthorized parking of motor vehicles on the other. Some legal crimes are also moral offences ; some are adjudged to harm a particular community but not another ; some are not moral offences at all. Moreover, crime involves responsibility, and the most learned psychologists know very little about responsibility. The penal system imposes certain punishments for certain offences, and only occasionally is the punishment meted out with respect to the need of the offender. Such confused territory is particularly in need of clear and reasoned guidance. Not all the criminological literature that comes from the United States can be recommended to students. That large country has been less successful in solving its own criminal problems than many others, and its national temperament perhaps too easily tolerates the emotional element in scientific inquiry. Professor ALBERT MORRIS'S book[3] sets a much higher standard, and is a real contribution to criminological knowledge. He takes the sound point of view that there are no crimes but only criminals, and so he writes about criminals and about the persons—not the institutions—who deal with them from their earliest childhood until after they are released from prison—policemen, lawyers, judges, and prison guards. He avoids, moreover, the common error of making the terms "prisoner" and "criminal" interchangeable, and includes in his study the criminals who are not in prison. His aetiology is well worked out: he traces the influence of the biological factor, and gives a scathing description of those conditions at large which influence criminal behavour ; he deals with the manufacture of the criminal from childhood, through his misadventures into community life, to his confirmation as a habitual criminal.

Fully a third of the book deals with the work of reformation and with the future of penology. Its keynote is just that quality of objectivity which the subject so badly needs. Professor Morris is seldom content merely to state a fact dogmatically ; he will nearly always follow it up with an interesting and revelant example from the newspaper, from some other work, or from his own observation. To read his book is something like witnessing a succession of extracts from cinematographic films of the underworld not only of America, but of most other countries in most other ages. His readers will not only be interested throughout, but they will gain from him a working notion of "what criminology is all about," what goes to make up the real criminal, and the lines along which crime might ultimately be abolished.

[3] *Criminology*. By Albert Morris. London: Longmans, Green and Co. 1934. (Pp. 590. 16s. net.)

USEFUL DRUGS

The Council of Pharmacy and Chemistry of the American Medical Association performed an important service to therapeutics when it prepared the pocket volume entitled *Useful Drugs*.[4] This first appeared in 1913, and we now have much pleasure in welcoming the ninth edition.

The purpose of the volume is to provide in a short form an account of the nature and action of those drugs which are of real service to the practitioner of medicine. In accordance with this principle a rigid censorship has been exercised at each revision, and by deletion of obsolete drugs the volume has been maintained at a constant size. The present volume contains about 350 monographs, which occupy 170 pages. Great care has been taken both in the selection of the drugs noticed and also in the writing of the monographs, and the latter provide a condensed but authoritative account of the pharmaceutical and pharmacological properties, together with the therapeutic uses of the drugs chosen for notice.

The present edition shows few outstanding changes from the last one, but the monographs have all been carefully edited. The editors are Professors R. A. Hatcher and C. Eggleston, and they are to be congratulated on the care with which they have carried out their work of revision. The volume deserves to be better known by the medical profession in this country, for it is of small pocket size, is very cheap, and yet provides an adequate account of nearly all the drugs really needed in the practice of medicine.

GENERAL PATHOLOGY

There is more than a suspicion that some lecturers use their notes year by year with little, if any, alteration. Such a suspicion could not be entertained by the students attending the lectures of Professor NOEL FIESSINGER, who holds the chair of experimental and comparative pathology at the Charité. Setting out to re-dress his facts for his course of lectures in 1933–4, he has envisaged the biochemical and pathological processes as a series of transformations, and thus has produced a stimulating and readable volume.[5]

Disease may be considered as a conflict in which an organism is defending its integrity against an external agent. The animal cell, whether in a unicellular or multicellular organism, may act in this conservative process either by utilizing or not the external object that presents itself, and in the latter case the non-utilization may be either a refusal or a rejection of the object. The author then develops this thesis, by discussing the transformations that occur in the animal body of the proteins, carbohydrates, and fats of the foodstuffs, and the water and salts which accompany them in the food. In this way the fundamental biochemical processes of the normal, and more especially the diseased, organism are reviewed, and by further development the whole range of pathological chemistry is covered. Attention is given not only to the metabolic substances ingested with the food, but also to what is described as the internal metabolism of the blood-colouring matters and their change into bile

[4] *Useful Drugs*. Edited by Robert A. Hatcher, Ph.M., Sc.D., M.D., and Cary Eggleston, M.D. Ninth edition. Chicago: American Medical Association. 1934. (0.60 dollar.)

[5] *Physiopathologie des Traversées Chimiques et Bactériennes dans l'Organisme*. By Noel Fiessinger. Paris: Masson et Cie. 1934. (Pp. 370 ; 6 figures, 4 plates. 45 fr.)

and urinary pigments. By further extension the theme is made to cover the bodily reactions to inanimate particles, and so the reader is led to study bacterial infections and toxins both of animal and of mineral origin. Finally, the transformations which occur in the ground substance itself, the body cell, and which produce the phenomena of anaphylaxis and allergy, are dealt with in a highly interesting manner.

All through, the clinical implications of the reactions described are kept before the reader, and the book thus forms a valuable link between the hard facts of biochemistry and their application to bedside diagnosis. It is easy to read, and covers the ground most successfully.

BRITISH HEALTH RESORTS

We warmly commend, both to medical practitioners and to the public, the latest (1935) edition of *British Health Resorts*, the official publication of the British Health Resorts Association. First issued only two years ago, it has, in this short period, established itself as the only completely authoritative guide for the doctor seeking the most suitable spa or seaside resort for a patient and for members of the public desiring a healthy and congenial place for permanent or temporary residence. The information, compiled from official sources, has again been extended ; one new section deals with the value of British watering-places as winter resorts. Comparison has been made with the climate of the all-too-popular Continental resorts, and here it is pointed out that the average temperature from November to March is 24° in the Alps, 45° in South-West England, and 49° at Nice, and that whereas the daily range of temperature, a matter of chief importance for invalids in northern countries, is 19° in the Alps and 18° at Nice, it is from 10° to 12° in the West of England. But the handbook is not concerned only, or even chiefly, with indications of climate. The editor and others responsible for its compilation have recognized that people who go to health resorts look for comfort and amusement as well as for treatment in healthy surroundings. They have therefore included all essential information concerning local attractions and amusements. Messrs. J. & A. Churchill are the publishers from whom it can be obtained, price 1s., or from the offices of the British Health Resorts Association, 197, Piccadilly, W.1.

Notes on Books

The first edition of *Diagnostics Urgents : Abdomen*, by H. MONDOR, was reviewed in these columns three years ago. A second edition has now made its appearance, revised and brought up to date.[6] It consists of two handsome, well-printed, and very well illustrated volumes, in which the diagnoses of all the acute abdominal and pelvic catastrophes are very fully and completely set out. Dr. Mondor stresses the point that, in the acute abdomen, correct diagnosis makes all the difference between life and death. Symptoms and signs cannot be watched from day to day but, in cases of doubt, from hour to hour. It is not the number of the physical signs which matter but their quality ; they are not to be counted, but the significance of each very carefully weighed. In these volumes the methods of examination to elicit the physical signs, and the proper interpretation of them when elicited, are described exhaustively, and nothing has been left out. There is an abundance of good illustrations, both photographic and diagrammatic. The radiographic figures have been particularly well chosen, and in many instances the interpretation of them is demonstrated by an inset diagram showing the special points to be noted. The author is to be congratulated upon the production of such an excellent work.

[6] *Diagnostics Urgents : Abdomen*. By H. Mondor. Second edition. Two volumes. Paris: Masson et Cie. (Pp. 1064 ; 276 figures. 160 fr.)

One of the curious features of human life is that the ways of a man's soul are so diverse that only very seldom can the path taken by one traveller serve as a useful map for others on the way. This is partly because individual experience is only of real value when it is found out *de novo*, and without assistance. The traveller who has made his way through what was to him a trackless forest has achieved something quite different from the one who takes the same path with the aid of maps and guide-books compiled by others. The process by which Miss JOANNA FIELD found her way to the synthesis which she describes as happiness could have been taught to her in more than one esoteric and psychological school. Yet had she learnt it at the feet of a teacher she would have been less rich as a result, and would have had little stimulus to compile the book which she has published. The material she courageously offers in this small volume, *A Life of One's Own*,[7] is of great interest to the psychologist, and may well serve to stimulate others to follow in her footsteps and seek for themselves the path to a fuller life. Her description of the conflict of the opposites which she came ultimately to characterize as maleness and femaleness, and the synthesis which she has managed to achieve, will be of interest to many readers. She writes simply and straightforwardly, and sets forth her progress exactly as it happened, from the first vague realization that she was not as happy as she might be, to a final interpretation which is far more in harmony with the teachings and discoveries of Jung than she herself fully realizes.

The Department of Scientific and Industrial Research has issued No. 1 of the sixth volume, dated March, 1934, of its *Index to the Literature of Food Investigation* (H.M. Stationery Office, 5s.). The fifteen classified sections are preceded by a general review of developments during 1932–3, and a full index of authors is appended.

The numerical summary in the *Medical and Dental Students' Register*[8] for 1934 shows that 2,350 medical students registered last year, representing an increase of 63 over the figure for 1933, and of 403 over that for 1932 ; the entry was, in fact, the largest recorded during the past fourteen years. The number of dental students registered in 1934 was 422, an advance of 39 over the figure for the previous year, and the largest entry since 1922.

[7] *A Life of One's Own*. By Joanna Field. London: Chatto and Windus. 1934. (Pp. 252 ; 3 plates. 7s. 6d. net.)

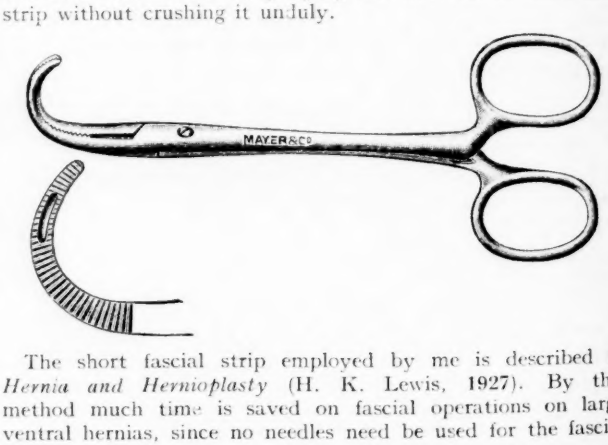
[8] Published for the General Medical Council by Constable and Co., Ltd., 10, Orange Street, W.C.2.

Preparations and Appliances

FASCIOPLASTY FORCEPS

Mr. ERNEST COWELL, F.R.C.S. (London, W.1), writes:

Messrs. Mayer and Phelps have made for me a pair of forceps for use in fascial suture. This instrument has its end curved like an aneurysm needle, and is made to push through the tissue under suture, grasping the free end of the fascial strip without crushing it unduly.



The short fascial strip employed by me is described in *Hernia and Herniоplasty* (H. K. Lewis, 1927). By this method much time is saved on fascial operations on large ventral hernias, since no needles need be used for the fascial strip.

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CLIMATE AND LIFE

Unexplained facts of common experience may yet be axioms of unassailable validity. The compelling influence of physical environment on organisms is a fact of this order. From early times body and mind, health and disease, were firmly believed to be subject to cosmo-meteorologic influence, although the explanations offered, from astrology downwards, were generally dogmatic and irrational. With the scientific revolution in medicine a great many theories, which were often only erroneous interpretation of facts, were swept aside and gave place to a negative and even a nihilistic reaction, with an inability to accept facts which appeared to be inexplicable. A more scientific attitude is now apparent, especially in the face of facts which are known but not understood.

It is known that life is actually immersed in and dependent upon two milieus, which are respectively within and without the body. The *milieu intérieur* of Claude Bernard is always the same: any measurable change in that inner atmosphere is almost incompatible with life. Barcroft has lately described[1] the astonishing physiological mechanisms which in a warm-blooded animal preserve its unvarying constancy of temperature and composition. Outside the body is the *milieu extérieur,* equally necessary as a continuous support of life. But this is a changing environment, including many forms of energy, to which again the warm-blooded animal has so intimate a power of adaptation that every external influence evokes its own response. When by failure of normal reactions the equilibrium with the external world is weakened or broken a new phase of life begins, and functional perturbations lead on to more permanent pathological states. So closely are animal and vegetable forms limited by physical barriers that Köppen has proposed to classify climates according to the prevalent species. For example, in the northern temperate region there is the climate of the oak ; further south that of the olive, and, where the summer is more rainy, that of the camellia ; on humid and especially Atlantic shores there is a climate of the fuchsia. Animals also show well-recognized changes of structure and habit according to temperature. The great increase of adipose tissue, slowing of vital processes, with hibernation and a rapid awakening of life in the spring, are Arctic characters.

A remarkable book has appeared under the editorship of Dr. Piéry of Lyons,[2] consisting of more than one hundred monographs by leading authorities in different countries, dealing with the rather unfamiliar elements of biological and medical climatology. As Professor D'Arsonval says in his preface, climate can be studied from two entirely different aspects. The varied composition of a climate according to season and place is a physical problem ; the reaction of the organism is a biological or medical problem. This cardinal distinction has often been overlooked. Although older writers drew attention to the profound biological significance of climate, recent authors, impressed by the exactitude of modern atmospheric records and their value in navigation and aviation, have consistently followed the example of Hann in his classical work on climatology (1883) in basing their determination of climates almost solely on meteorological data. For this reason climate has come to be considered too exclusively the province of the meteorologist. It may be noted, however, that, historically, a knowledge of climatology founded on biological data began long before measuring instruments, and that a biological problem can never be solved by meteorological studies. Anemometers, thermometers, hygrometers, register *single* elements or factors of climate. The organism, with an equal or greater sensitiveness, responds to the complex of climate *as a whole.* Medical climatology is therefore inspired by what has been termed *physical biology,* a comparatively new science, which has rapidly advanced of late years as the result of a variety of investigations in many countries. The recent progress in the medical applications of radiant energy is a case in point. In France, Germany, Switzerland, and Czechoslovakia bio-climatic research institutes have been founded. Among other subjects that are being explored are the effects of climate upon thermo-regulation and upon the cooling and desiccation of the body, upon the respiratory exchanges, nutrition, the circulation, and nervous tone. The effect of weather changes on the course and character of chronic diseases is another objective. It is claimed that the conditions for the medical uses of external energy may before long be accurately determined, and that a scale of climatic values may be established. What, after all, have the organs to do with the freezing and boiling points of water? But there are organic scales of temperature, humidity, and light, affecting alterations in protoplasm, the adaptation to external temperature, and many other functions. What, again, is the thermobiologic zero, and what are the critical points?

Thanks especially to English physiologists and Swiss observers, new biological measurements are being established. Häberlin, to whose work we have already called attention,[3] has demonstrated the reactions of delicate children to a tonic marine climate. Leonard Hill has shown the enormous increase of metabolism in children exposed to the fresh air, especially at the

[1] *Features in the Architecture of Physiological Function.* By Joseph Barcroft, F.R.S. Cambridge, 1934.

[2] *Traité de Climatologie biologique et médicale.* Directeur, Professeur Piéry. Secrétaires de la Rédaction, Docteurs Milhaud et van der Elst. Préface du Professeur D'Arsonval. 3 tomes. Paris: Masson et Cie. 1934. (Pp. 2,664. 330 fr.)

[3] *British Medical Journal,* 1931, ii, 268, 331, 348.

seaside and in the mountains. Comparing the results observed at Hayling Island and in the Swiss Alps, he concludes that metabolism can be excited by exposure and bathing in southern England to the same degree as in an Alpine climate. Types of respiratory, digestive, muscular, and cerebral function corresponding to differences of climate are suggested in the comprehensive treatise before us, which abounds in interesting illustrations of the new approach to climatology based upon biological and medical observations. The cooling power of a climate or of weather at any moment would appear to be an extremely variable factor. Certainly it is at a minimum at all seasons of the year in warm and equable and sheltered places in the West Country, and rises to a maximum in England in windy places on the East Coast. The vaso-constrictors, says Léon Fredericq, are the "guardians of the organism against the invasion of cold." It is generally admitted that the British climate, with a continual alternation of external temperatures and corresponding oscillations in body cooling, is the best in the world for the production of physical and mental energy. The happy warrior has a normal resistance to cold. It is far otherwise with frail and chilly persons, and weather changes in the Alps have been shown conclusively to accelerate the evolution of bacillosis in the lungs or bronchi, and of Bright's and rheumatoid disease. Parkes Weber suggests that circulating bacilli may be arrested in tissues injured by cold. Such considerations show the importance of measuring the limits of resistance to cold in invalids. A beginning has been made in regard to rheumatic patients, in whom the production of heat by muscular activity is handicapped by immobility.

Another factor deserving of detailed study by the medical climatologist is the morbidity of winds. It is recognized that winds range from a gentle stimulus to strong excitation, and that each particular wind brings with it the characteristics of the climate from which it blows. For example, the *vent du Midi* is dry and hot, and withers the leaves of trees, while pipits and warblers lose one-third of their weight. The profound dehydration in infants when this wind blows has been studied by Mouriquand. In temperate climates winds may clearly diminish or increase the incidence of those diseases in which chill, excess or defect of humidity, nervous excitation or congestive states, are factors of causation. In the British Islands the moist sea winds (unless very strong) are held by the authorities here cited to be "favourable to the organism." Serr of Clermont-Ferrand speaks of three types of climate in Continental countries as favourable for debility and diminished vitality (*climats de réparation*): the almost subtropical climate of the Mediterranean winter health resorts, that of the summer resorts on the Atlantic littoral, and the climate of mountains in both winter and summer. There is no lack of data in the British Islands. If the experience of medical men, especially in convalescent homes and schools, were confirmed by measurements and records a differentiation of the British *climats de réparation* could be readily established, going from east to west, according to season, prevailing winds, humidity, and temperature. It is evident that in the British Islands a wide field is open for the systematic utilization of these powerful tonic and sedative influences of the east and of the west, particularly in the case of delicate children and in advanced life. The physician has to follow the scientific agriculturist and to consider what may be in any particular case the optimum environment for the development or the protection of the human plant. Among the accessory methods of treatment employed at the seaside, the medical values of which require rigid investigation, are hot and temperate sea-water baths, douches, eaux mères, pulverization, sea-weeds, and estuary muds. For this and much other research work an institute of bio-climatology is surely needed. It might also collate the reliable data in each climatic region, and bring the best knowledge and guidance within the reach of the medical profession and public; and above all it might give a new and medical orientation to the study of climatology in the British Islands.

SURGICAL SHOCK

The outstanding feature of primary shock is a fall in arterial pressure, but if the fall is such that the blood pressure remains at or above 80 mm. Hg the oxygen requirements of the tissues may still be satisfactorily met. If, however, the blood pressure falls to about 70 mm. Hg, and is maintained at this level for some time, secondary shock supervenes, and the consequences are extremely serious. It is clear that there will be a diminution in oxygen supply to the tissues, with consequent fall in metabolic rate: the skin is cold and clammy, the pulse is thready, and the heart beats rapidly; respiration is shallow and poor; the patient is weak and toneless; thirst is experienced, but the patient may be unable to retain fluid in the stomach; finally, unless adequate measures are taken, the outcome is fatal. From the point of view of aetiology one need only consider primary shock, since the phenomena of secondary shock call for no special explanation, as they are the normal and anticipated consequences of a maintained extreme hypotension.

In a recent series of papers[1] a valuable account has been given by a number of observers of modern views on primary shock. Professor W. B. Cannon presents a valuable review of the evidence for various theories. He rightly points out that a weakened heart is not a serious aetiological factor. The heart, even in such an emergency, is well able to make the additional effort required of it. Discussing the commonly enunciated view that shock is due to loss of vasomotor tone (with consequent stagnation of large volumes of blood in the splanchnic region) Cannon draws attention to the fact that the fall in arterial blood pressure is responded to by a vaso-constriction through the carotid sinus. It is, of course, conceivable that a long-continued tissue anoxaemia may lead to such malnutrition

[1] *Annals of Surgery*, October, 1934.

of nerve cells that a loss of vasomotor tone may occur, but it seems probable that by that time the situation is one of deep secondary shock. One of the main causes of shock seems to be in the reduction of blood volume, on which all observers are agreed. In some at present rather obscure way the blood volume is reduced below the minimum capacity of the vascular system, so that the vessel walls cannot contract down on the blood. The modes of production of this remarkable loss of blood fluids are the bases of several theories of causation. The theory that this is brought about by a toxic, histamine-like substance liberated from injured tissues is an old one, and dates from war-time observations. While there is still a considerable school of support for this view, the newer observations of several American workers have shown that the hypothetical toxic factor is not detectable (indeed, muscle tissue is remarkably poor in histamine) and that the blood lost from the circulation is really present in the injured tissue itself. The toxic theory cannot be dismissed merely on the grounds that muscle is poor in histamine, for there are certainly other powerful vaso-dilators in the muscles, and these might conceivably be concerned with the early stages of shock.

The probable involvement of the autonomic nervous system as an aetiological factor is suggested by a large number of facts—for example, pain and fear may bring on symptoms like mild shock ; abdominal operations with manipulation of the viscera (hence stimulation of sympathetic afferents) may cause a fall of blood pressure to shock levels. Experiments by Freeman have demonstrated that stimulation of the sympathoadrenal system may produce the shock syndrome, with fall in blood pressure and loss of circulating fluid. It must be remembered that sympathetic stimulation, while causing splanchnic and cutaneous vaso-constriction, produces dilatation of the vessels of the muscles—a state of affairs that might readily allow of the passage of fluids into the muscles. Nevertheless, it is difficult to see how so complex a matter as filtration of blood plasma into muscles could explain the collapse which every medical man has seen in some cases on merely showing the patient a syringe with which blood is to be drawn. Psychogenic shock of this type seems surely to be related to some immediate and considerable diminution of the cerebral circulation ; the consequent temporary collapse by serving to remove the psychological stimulus allows of correspondingly rapid recovery. How this type of shock is produced is unknown. Some experiments by Heuer and Andrus[1] are of interest. These workers observed that aqueous extracts of fatally strangulated intestinal loops could, on intravenous injection into dogs, give rise to symptoms of shock, vaso-dilatation and loss of plasma from the blood. This effect would be antagonized by extracts of adrenal cortex, which is of great interest in view of the importance of the adrenal cortex in regulating the exchange of fluid between blood and tissues.

The special problem of traumatic shock following violent blows to a limb has recently been very carefully studied by O'Shaughnessy and Slome[2] in the anaesthetized cat. Using many modern methods of physiological research it was possible to demonstrate conclusively that " a toxaemia due to the elaboration of histamine, or any other depressor substance in the traumatized area, plays no part in the syndrome of traumatic shock." These authors make a strong case for local fluid loss and nervous stimuli in the aetiology of shock. Spinal anaesthesia before the injury delayed the onset of shock in spite of great fluid loss ; also, spinal anaesthesia induced during the period of extreme circulatory failure led to marked recovery of blood pressure. This noteworthy paper, in conjunction with the findings of several American workers, accentuates the immense importance of the elimination of nociceptive nervous stimuli in the prevention and treatment of traumatic shock.

MEDICAL BENEVOLENCE

The Royal Medical Benevolent Fund has entered upon its centenary year. It was in 1835 that the Benevolent Fund of the Provincial Medical and Surgical Association took definitive form, and for many years it was " a great and integral part " of what is now known as the British Medical Association. Not until 1870 did the charity assume the title of the British Medical Benevolent Fund, and, without losing the good will of the Association, cease to report to its annual meetings. The Fund has placed a self-denying ordinance upon itself in connexion with its centenary, for it will make no appeal by way of a public dinner or other function or a broadcast ; its appeal will be made only to members of the medical profession, and not until the fullest possible support of the profession has been enlisted will it ask the aid of the general public. Its aim is not to raise in any particular year a specially large sum of money, but to secure a larger regular income by enrolling a dependable body of annual subscribers, and the administrators intend between now and 1936 to invite all members of the profession to subscribe.

Such was the announcement made at the annual general meeting of the Fund, held under the chairmanship of Sir Thomas Barlow on March 27th. The annual report stated that there was some confusion on the subject of medical charities, which it proceeded to clear up. The charities are the Royal Medical Benevolent Fund, together with its Ladies' Guild, the Royal Medical Foundation of Epsom College, and the Sir Charles Hastings Fund of the British Medical Association, and in Ireland the Royal Medical Benevolent Fund Society. Between these bodies there is the closest co-operation in the way of interchange of information regarding applicants and beneficiaries and the avoidance of overlapping appeals. The Charities Committee of the British Medical Association seeks to arouse greater interest in medical benevolence, and is a collecting agency, allocating any sums which it

[2] *Brit. Journ. Surg.*, 1935, xxii, No. 87.

receives among the existing charities. The accounts of the Royal Medical Benevolent Fund, which were presented by the honorary treasurer, Dr. Lewis G. Glover, showed the book value of the investments to be £172,656, and the income received on them £7,333. During the year £12,755 was expended in ordinary and special grants, £5,369 in annuities, and £1,291 in social service and administration.

Mr. R. M. Handfield-Jones, the honorary secretary, reported that during the year 869 new subscribers had supported the Fund, and the total received in subscriptions and donations was £11,092, and in legacies £9,726. The number of persons directly connected with the medical profession who had been helped financially in 1934 was 670. Examples of types of cases dealt with by the committee were given: practitioners who could no longer work owing to age or illness and whose savings were exhausted; widows whose incomes had gradually fallen by the reduction or non-payment of dividends to a point where the necessities of life could no longer be obtained; daughters of medical men who had bravely supported themselves since their fathers' death, either by nursing or teaching, or even by domestic service, until age or ill-health had rendered them unemployable; and, most pathetic of all, young widows with infant children left unprovided for by the untimely death of the bread-winner. Sir Thomas Barlow was re-elected president, Dr. Glover and Mr. Handfield-Jones respectively honorary treasurer and honorary secretary; to the existing vice-presidents twenty-one fresh names were added; and the members retiring by rotation from the committee of management were re-elected. On the motion of the president, a vote of thanks was accorded to the British Medical Association and its Charities Committee, to the Ladies' Guild, to various honorary officers, and to the medical and lay press for much useful publicity.

THE BRONCHIECTATIC SEPTIC TANK

Advances in the treatment of bronchiectasis have recently been made in two directions. First, the condition has been recognized in its incipient stage in wider groups of cases, so that treatment by prophylactic management, vaccines, and postural drainage may be combined to prevent the chronic suppurative condition which we know to be so dangerous and so unpleasant. The second line of treatment has been in radical removal of the diseased lobes, for it is now known that saccular bronchiectasis cannot be cured by other means, and that collapse therapy in this disease rests on no sure histological rationale. A few exponents of bronchoscopic drainage have been enthusiastic enough to lay claim even to the possibility of cures being obtained by this means. There has, however, been general disappointment with its results in established bronchiectasis, so that it has hitherto been used chiefly as a palliative in those advanced cases where it forms the only method of relief. In their paper on this aspect of the diseases[1] the Chevalier Jacksons introduce a rather different outlook, which promises better results. They draw a parallel between bronchiectatic cavities which act not only as receptacles for septic materials, but also as containers "where bacteriological processes change the character of the contents in a way comparable to the process in what sanitary engineers call a septic tank." Their unique experience in the study of secretions removed by bronchoscopy convinces them that at the outset the bronchiectatic secretion is extremely viscid, and that at this early stage removal of the viscid pus with a powerful aspirator will forestall the septic tank process that would otherwise ensue from "the inability of the cilia, cough, and tussive squeeze to expel this material." Treatment may then be carried out along the usual lines with much more likelihood of success. This appears to rest on an intelligent recognition of the fact that disease processes can only be altered at particular moments in their course. The trouble with results from bronchoscopy has not been that bronchoscopy is a poor form of treatment, but that it was not applied at precisely the right time. These workers claim that their observations add one very definite aetiological factor to the usual list, containing congenital conditions, changes in the bronchial wall, obstruction, etc.—namely, this viscosity of certain pathological secretions. When a mucous membrane such as the bronchial mucosa is kept in contact with any irritant material, pus is produced. We are becoming much more cognizant of the principle of chemical suppuration, and in this region excellent examples are recognizable in the biochemical processes by which sacrophytic organisms are active in the reduction of viscosity. The unusual secretions arising perhaps from primary bacterial infection, perhaps from chronic irritation, stimulate the mucosa and produce pus swarming with organisms which are obviously not primary. Inoculation of the stagnant pus in the tracheo-bronchial tree is obviously by putrefaction by way of the inspired air and inspiration of secretions of the mouth during sleep. Bacteriological studies of bronchiectasis are well known to be unsatisfactory and inconclusive. This is not entirely due to oral contamination, as is generally supposed. An important fact not usually taken into account is that the primary pus is not expectorated at all on account of its high viscosity: it is only coughed up from the "septic tank" when its viscosity is reduced by putrefaction. The observations upon which these conclusions rest have been derived partly from bacteriological studies made from tissues removed at lobectomy, and also from bronchoscopy. The formidable list given in this paper of routine observations to be made when viewing the bronchi shows the thoroughness with which the work is carried out in this well-known clinic. Finally, a recognition of the importance of this viscosity factor gives us an obvious reason for avoiding the use of drugs such as atropine, which will thicken the secretion, and of opium derivates, which will check the cough reflex in early bronchiectasis. "On the contrary," say the Jacksons, "when the patient feels that a paroxysm of cough is coming on he should welcome it, immediately assume the most favourable posture, and otherwise assist the expectorative efficiency to the utmost."

[1] *Amer. Rev. Tub.*, December, 1934, p. 599.

ESTIMATION OF DUST IN AIR SAMPLES

The accurate estimation of the dust particles in the air to which certain industrial workers are exposed is of great practical importance, as may be judged by the fact that silicosis causes over 300 deaths every year in England and Wales. A report on the subject[1] by H. L. Green and H. H. Watson, who have carried out their investigation under the Committee on Pulmonary Disease set up by the Medical Research Council, is therefore very welcome. The investigators, who were lent from the staff of the Chemical Defence Experimental Station at Porton, confined their inquiry to the occupation of the stonemason. They found that three well-known types of instrument hitherto used for estimating dusts had an efficiency of only 40 to 64 per cent., so they employed a new instrument, the thermal precipitator, which had been developed by Professor R. Whytlaw-Gray and Dr. R. Lomax. In the improved instrument used by the investigators a measured volume of air is caused to stream slowly past an electrically heated wire, and it deposits its dust uniformly on a cover slip. It is said to be practically 100 per cent. efficient for all particles likely to be of pathogenic importance, and samples can be taken over periods of time from ten minutes to two hours or more. It was found that 80 to 90 per cent. of the particles inhaled by the stonemason in the course of his work are less than 2 μ in diameter, and 35 to 45 per cent. of them are less than 0.5 μ; only 1 per cent. of them are over 5 μ. The numerous observations made under industrial conditions indicated that it is undesirable for masons to work in enclosed yards. They ought to work in the open, so as to allow natural ventilation to take full effect. On days when there is a dead calm the exposure may be three times greater than on a windy day, and the mason is exposed to very high concentrations of dust when he is punching the stone. It follows that, if he objects to wearing a respirator continuously, his exposure to dust would be considerably reduced if he wore one whilst punching on calm days. Alternatively the stone might be worked wet, as the dust then thrown up is greatly reduced.

[1] Special Report Series, No. 199. Medical Research Council. Physical Methods for the Estimation of the Dust Hazard in Industry (with Special Reference to the Occupation of the Stonemason). By H. L. Green and H. H. Watson. London: H.M. Stationery Office. 1935. (1s. net.)

PHYSICIAN—ANTIQUARIAN—DIVINE

The March issue of *Antiquity* contains a lively account by Mr. Stuart Piggott of William Stukeley, the physician, antiquarian, and revivalist, who was born in 1687. He was educated at Corpus Christi College, Cambridge—where he studied chemistry, botany, and medicine, and collected material for a new edition of Ray's *Catalogus Plantarum*—and completed his medical training at St. Thomas's Hospital. After qualifying M.B. in 1709 he practised first at Boston (Lincs), and in 1717 came to London, graduating M.D. in 1719. He was elected F.R.S., and later was placed on the council and appointed one of the committee to examine the condition of the instruments in Greenwich Observatory. Shortly after settling in London he joined a dining club at the Mitre Tavern in Fleet Street, which he was largely instrumental in converting into the Society of Antiquaries of London, of which he was the first secretary. In striking contrast with the ridiculous theories he promulgated during the last thirty years of his life, which gained for him the name of the Archdruid of his age, he carried out between 1718 and 1725 a series of investigations at Avebury and Stonehenge, which, according to Mr. Piggott, show him to have been the first field archaeologist that England had so far seen or was to see for a century. He was thus, Mr. Piggott remarks, one of the first of the large body of medical men who turned their scientific training to the study of archaeology with excellent effect. In 1729, worn out by the fatigues of practice and by repeated attacks of gout, and fired with religious enthusiasm in opposition to the prevailing scepticism of the age, he became ordained at Croydon and was appointed to the living of All Saints, Stamford, which he afterwards changed for that of St. George's, Holborn. We learn, however, from Monk's Roll that he retained his interest in the Royal College of Physicians, of which he had been elected Fellow in 1720, Goulstonian lecturer in 1722, and Censor in 1725, and that he was a regular attendant at the comitia until his death in 1765. Stukeley was perhaps best described by Warburton, Bishop of Gloucester, as "a learned and honest man, but a strange compound of simplicity, drollery, absurdity, ingenuity, superstition, and antiquarianism."

ECLAMPSIA AND THE PITUITARY GLAND

Anselmino and Hoffmann[1] have brought forward experimental evidence in favour of the view that eclampsia and pre-eclampsia may be attributed to hyperfunction of the posterior lobe of the pituitary gland. Working with rabbits, they found that the ultrafiltrates of blood withdrawn from patients suffering from either of these two conditions contained both a pressor and an antidiuretic substance. In view of the fact that some of these ultrafiltrates also caused expansion of the melanophores of a frog, they assumed the identity of the substances in the ultrafiltrates with those in post-pituitary extract. This work has received much attention, and has secured the support of no less an authority than Harvey Cushing,[2] who has advanced the thesis that certain hypertensive states, including eclampsia, are to be attributed to basophilic hyperplasia of the posterior lobe of the hypophysis. It is therefore of interest that two recent papers, the one by Byrom and Wilson[3] and the other by Theobald,[4] should have been devoted to a critical survey of the work of Anselmino and Hoffmann. Byrom and Wilson repeated their experiments, but found the rabbit such an unsatisfactory animal with which to work that they placed no reliance on their results. They therefore used the rat method, introduced by Burn for the assay of post-pituitary antidiuretic substance. Their results were clear-cut, for on no occasion did 1 c.cm. of the ultrafiltrate from any toxaemic patient contain as much antidiuretic substance as is present in 0.002 of an international unit of pituitrin. Byrom and Wilson therefore concluded that "if an excess of antidiuretic

[1] Anselmino and Hoffmann: *Arch. f. Gynäk.*, 1931, cxlv, 95 and 114; and cxlvii, 597.
[2] *Amer. Journ. Path.*, 1933, ix, 539.
[3] *Quart. Journ. Med.*, 1934, xxvii, 361.
[4] *Clin. Sci.*, 1934, i, 225.

principle is present in the blood of toxaemia, its concentration must be considerably lower than Anselmino and Hoffmann suggest." Theobald points out that the hypothesis under discussion is based on the assumptions that the antidiuretic is distinct from the pressor substance, and that these substances may be secreted by the post-pituitary gland in widely varying proportions, for neither of which assumptions there is, he contends, any reliable evidence. Unlike Byrom and Wilson, Theobald did find an antidiuretic substance in the ultrafiltrates of blood from eclamptic women, but in lower concentration than the lowest estimations of Anselmino and Hoffmann. Seeing, however, that it passed through a membrane known to be impermeable to post-pituitary extract, it could not have derived from the hypophysis. It is a little unfortunate that Byrom and Wilson did not inject larger doses of their ultrafiltrates into the rats, for while their conclusions do not conflict with those of Theobald, the important question whether an antidiuretic substance may be obtained from the blood of such patients must remain undecided. It is, however, clear that neither paper offers any support to the hypothesis of Anselmino and Hoffmann, for if the antidiuretic substance found by them is identical with that found by Theobald it is obvious that it did not derive from the hypophysis.

THE SYSTOLIC MURMUR

The relation between a systolic murmur and valve disease is far from strict. It is probably safe to say that as often as not a systolic murmur heard at the apex of the heart is not associated with valve disease, and certainly this is true of murmurs in systole heard at the base. It is also a sound generalization to say that an apical systolic murmur in the absence of symptoms and any other signs may be ignored. Conversely, with mitral valve disease accompanied by severe failure no apical murmur may be audible. Clinical experiments on the production of systolic murmurs have been made by R. D. Friedlander and M. G. Brown[1] on one hundred patients free of heart disease. In four of these after the inhalation of amyl nitrite a murmur appeared systolic in time and most commonly at the pulmonary area. It was never of more than slight intensity, and usually became audible at a time when the pulse rate was declining and the blood pressure had recovered from the initial fall and had risen above the resting level. Of twenty patients with fever, in some artificially produced, only four developed a murmur, and the average blood pressure and pulse rate changes in these did not differ notably from similar changes in those in whom no murmurs were detected. Transient murmurs were noted in some patients with anaemia, though in others with comparable blood pictures none was heard. In anaemia the rate of blood flow is increased, a change also known to occur in toxic goitre, a condition in which systolic murmurs may be heard. In neuro-circulatory asthenia, however, though the pulse is quick and a systolic murmur common, the blood velocity is not greater than normal. It is suggested by Friedlander and Brown that here the sharp first sound often heard indicates an increased rate of ventricular discharge and increased blood velocity in the large vessels only. The sum of the findings recorded supports the old view that "functional" systolic murmurs chiefly depend on an increased velocity of blood flow leading to eddies in the blood current, and their greatest frequency at the pulmonary area is accounted for by the relatively greater distensibility of the pulmonary artery as compared with the aorta. It is pointed out that all the murmurs heard in the conditions studied were faint, and that loud murmurs will generally indicate organic disease ; but it is not the loud systolic murmur which in the past has caused so much confusion.

[1] *Ann. Int. Med.*, February, 1935, p. 893.

OPHTHALMOLOGY IN EGYPT

The annual reports of the Giza Memorial Ophthalmic Laboratory in Cairo maintain their high level of interest under the direction of Professor Rowland F. Wilson and Major F. H. Stewart.[1] The chief papers are those on climate and the seasonal incidence of acute ophthalmia, and the studies on trachoma. Seasonal variations in the incidence of conjunctivitis have been known for many years. Bishop Harman published graphs of London findings in 1905,[2] observations were made in Egypt by Meyerhof in 1911[3] and MacCallan in 1914.[4] Meyerhof noticed that there were two peaks during the hot season, a high in May and a lower in September. Recent investigations have shown the constancy of these findings, and it has been observed that the two periods of maximum incidence of Koch-Weeks conjunctivitis and the height of the gonococcal conjunctivitis epidemic coincide with the two principal fly-breeding seasons. The coincidence is so stable and so remarkable that it seems scarcely possible to do other than admit the relationship. The four papers recording research work into the pathology of trachoma are of special interest. Major Stewart concludes therefrom that trachoma is a specific contagious disease of slow insidious onset with an incubation period of not less than fifteen days for the follicles of monkeys. Prowazek-Halberstaedter bodies, free initial bodies of Lindner, and free elementary granules have not been observed in monkeys inoculated in the laboratory. Trachoma virus can survive three to four hours at room temperature in saline solution or bouillon, and it can survive repeated grinding and centrifuging. It perishes quickly in fluids of *p*H 6.4. It is killed by 50 per cent. glycerin at 37° C. in twenty-four hours. Filtrates through gradocol membrane or Berkfeldt V candles are not infective, although the former may contain enormous numbers of granules which closely resemble the granules of P.-H. bodies. In centrifuge experiments the virus is not removed from the deposit by repeated washing—in fact, washed trachoma tissue appears more infective than unwashed. The evidence against the granules of Prowazek as a cause of Egyptian trachoma is summed up as follows: They are not present in the uncomplicated disease in men ; they are absent or unrecognizable in the experimental disease of monkeys. In filtrates, granules resembling them are not infective.

[1] Eighteenth Annual Report of the Giza Memorial Ophthalmic Laboratory. Cairo: Schindler's Press. 1934.
[2] *Conjunctiva in Health and Disease*, London, 1905.
[3] *Arch. d'Ophthalmol.*, 1911.
[4] *Ophthalmic Hospital Reports*, 1914–15.

TREATMENT IN GENERAL PRACTICE

This article is one of a series on the management of some of the major medical disorders met with in general practice

TREATMENT OF DIPHTHERIA

BY

ALEX. JOE, M.D.

Apart from the special requirements of laryngeal diphtheria, the treatment of diphtheria may be summarized under the headings of general management, specific treatment, and the treatment of sequels and complications.

General Management

The keynote of this is the maintenance of the recumbent position and the complete avoidance of all effort. At first the patient should be given only one pillow, all movement and sitting up in bed being strictly forbidden. Daily soap-and-water enemata should be administered, and careful note made of the amount of urine passed and of albuminuria. Spoon-feeding should be carried out. Owing to the condition of the throat the patient at the outset will be on a milk diet, but this should be supplemented daily with glucose 3 to 5 ounces dissolved in water flavoured with lemon or orange juice. As soon as the throat permits, and in all except the very severe cases this will not be more than a few days, the diet should be increased by milk puddings, egg beaten in milk, custards, jellies, mashed potatoes, pounded fish, minced chicken, and indeed anything within reason so long as digestion is satisfactory. Local treatment of the throat has now largely been abandoned, except where there is necrotic membrane or superadded septic infection, when flushing of the throat with sodium bicarbonate solution (6 grains to 1 ounce) three or four times daily will help to clear it up.

In mild or moderate cases a second pillow may be given at the end of two or three weeks, and in the course of another week a third pillow. The patient is next permitted to sit up in bed for a short period, and the process is gradually extended until, about the fifth or sixth week, he may leave his bed to rest on a reclining chair or sofa. Then a few steps about the room are allowed, and finally, at the end of two months, he should be taking short walks. In the mildest cases the above programme can obviously be speeded up, but, on the other hand, in severe cases, or when cardiac irregularities have occurred, it is wise to keep the patient flat for seven or eight weeks, and in bed for about twelve. In every instance, with each simple advance away from the recumbent posture, the most careful watch should be kept for variations in pulse rate and rhythm, faintness, or nausea, and at the slightest sign of these the patient may require to be put back one or two stages. Careful nursing is essential, the various nursing operations, such as daily bed bathing and toilet of the mouth, being performed with the maximum of gentleness, and a careful watch must be kept for early signs of pulse irregularities and the disturbance of function characteristic of the various palsies. Reading, for those so inclined, need not be prohibited, but in view of the possibility of ocular pareses this should be in moderation.

Specific Treatment

The most important step to be taken in treatment is to give the patient an adequate dose of antitoxin at once. In estimating dosage the factors to be taken into account are the extent and severity of the local lesion, cervical adenitis, and the amount of toxaemia as estimated by prostration, pallor, and albuminuria, while the duration of the disease before the administration of antitoxin must also be considered. Age and body weight are generally disregarded in this country. Since it is believed that better results are obtained from a single dose of antitoxin administered at the outset than by the injection of successive fractions whose total is the same, an effort should be made to estimate the requirements of the case at the beginning ; but this is not always easy, even by the most experienced, and the practitioner should not hesitate to repeat his injection if he is uneasy about the adequacy of the amount already given. Mild cases with slight patching on one or both tonsils, with adenitis limited to a very slight enlargement of the tonsillar glands, and with no clinical evidence of toxaemia will need 2,000 to 8,000 units. Cases of moderate severity, in which both tonsils are well covered with membrane, with a tendency to spread to the faucial pillars, the palate, or the uvula, and in which a slight degree of toxaemia and moderate enlargement of the cervical glands are present, should receive 10,000 to 24,000 units. Severe cases, with extensive membrane in the fauces and nasopharynx, marked faucial oedema, profuse nasal discharge, great swelling of the cervical glands, and profound toxaemia, shown by drowsiness, prostration, waxy pallor, and albuminuria, should be given 30,000 to 60,000 units. It may not be out of place to mention here that extent of membrane may be an unreliable guide to the severity of the process, as some of the gravest cases may show relatively small amounts of membrane, limited to the inner surfaces of the tonsils. Faucial oedema, prostration, and cervical adenitis will, however, give the clue to the severity of these lesions.

Patients with laryngeal diphtheria will seldom require more than 16,000 to 24,000 units, but if severe faucial or nasopharyngeal lesions are present in addition maximum doses as outlined above should be administered. When the diphtheria is limited to the nose and there is no evidence of toxaemia 2,000 to 8,000 units will be sufficient. Wound diphtheria, occasionally met with in children, and conjunctival diphtheria are comparatively rare, but should be treated as moderate or severe cases.

The minimum dosage as outlined above for each grade of severity would be applicable to cases coming early under treatment on the first or second day, the larger doses being indicated for those coming under treatment on the third, fourth, or fifth day. Furthermore, the patient should be re-examined after eight or twelve hours, since a spread of membrane or a worsening of the toxaemia would necessitate a reclassification of the case and additional serum. While numerous clinical observations bear out that little benefit is likely to follow the administration of serum after the fifth day of disease, it should be clearly understood that antitoxin should never be withheld, however late the stage and however hopeless the case may appear.

ROUTE OF INJECTION

In mild cases the serum should be given intramuscularly into the vastus externus. In moderate cases a quarter or half of the serum should be given intravenously and the remainder intramuscularly. In severe cases no effort should be spared to inject intravenously the whole, or

at least the greater part, of the serum, but should this be impossible—and this will occasionally be the case in small children with collapsed veins and marked toxaemia—intraperitoneal injection should be performed to secure more rapid absorption than is possible by the intramuscular method. This is carried out with the ordinary syringe and needle, the latter being pushed through the anterior abdominal wall in the midline, about two inches below the umbilicus. The various commercial serums now on sale are invariably concentrated, each cubic centimetre containing about 2,000 units of antitoxin. With a 20 or 30 c.cm. Record syringe, therefore, the injection of very large doses without refilling is possible, but, if necessary, refilling can be done by leaving the needle in the vein after carefully detaching the syringe. As all diphtheria antitoxin on sale in this country must conform to the standards of potency and sterility laid down in the regulations made under the Therapeutic Substances Act of 1925, the practitioner may rest assured as to the quality of any antitoxin that he may be supplied with.

While I am satisfied that the system of dosage laid down above will give all the results that can be expected from the administration of diphtheria antitoxin, it must be stated that agreement on the subject of dosage is not complete. Some experienced clinicians consider that for the severe case a total dosage of 100,000, or even 200,000, units, the great bulk of which should be given intravenously in one injection, is necessary. Whatever the experimental, clinical, and statistical considerations underlying these varying points of view, it should be made quite clear that it is never wrong to inject too much antitoxin, and a practitioner in doubt as to whether he should give a larger or a smaller dose should invariably decide on the former.

Sequels and Complications

In severe toxaemia some benefit may be brought about by the administration of glucose. Following the work of Schwentker and Noel, Benn and his co-workers in Birmingham advocated giving insulin as well, but the recent work by Begg and Harries has indicated that there is no advantage to be derived from insulin. Glucose administration should be initiated at the onset of treatment by following up the intravenous serum with the injection of 20 grams of glucose in 50 per cent. solution without withdrawing the needle. Thereafter 100 to 200 grams of glucose should be taken daily by the mouth in lemonade; or if this becomes impracticable by reason of nausea, 40 to 100 grams in 50 per cent. solution are injected intravenously each day until the obvious signs of toxaemia have abated. The administration of glucose will require, therefore, to be continued for from one to three or four weeks.

HEART FAILURE

At the first sign of this the foot of the bed should be elevated. Vomiting and pain require the application of frequent hot fomentations to the heart region, and all feeding by the mouth should be withheld, thirst being alleviated by moistening the lips and tongue with iced water. Rectal and intravenous salines should also be tried, if the latter can be given without much disturbance of the patient. Every drug with any reputation as a cardiac stimulant has been employed, but with little success. In common use are alcohol, ether, strychnine, camphor, caffeine, atropine, ephedrine, adrenaline, and pituitrin, while more recently coramine has been tried. If restlessness is marked small doses of morphine (for example, grain 1/16 in a child aged from 5 to 10 years) may be cautiously employed.

Friedemann has recommended hot baths as helpful in the vasomotor collapse in diphtheria, the patient being carefully conveyed to a bath at 105° to 110° F. in a sheet and immersed for ten to thirty minutes. The traditional treatment of diphtheria in this country would probably render clinicians cautious in applying the method, but I have had the opportunity of trying this line of treatment in a modified form—namely, the application of the hot-air bath. This can readily be improvised from the ordinary bed cradle by fitting it up with two or three electric light bulbs and covering with blankets until the required temperature is attained. It is not claimed that the benefit is more than temporary, but in several cases I have seen a striking improvement in the colour and comfort of the patient, and it is conceivable that some patients may be tided over a critical period. In patients who recover from this complication the remote prognosis is generally considered very good, but it would be well to forbid strenuous exercises for a year after recovery.

PARALYSES

The post-diphtheritic palsies proceed to spontaneous recovery, and need no treatment beyond tonics such as Easton's syrup. In palatal paresis, owing to the patient's tendency to regurgitate fluids through the nose, semi-solids should be given. In pharyngeal paralysis feeding by the nasal tube should be resorted to, care being taken to prevent the aspiration of even a few drops of the feed into the larynx, otherwise a septic bronchopneumonia may be set up. Respiratory paralysis is a hopeless condition unless early admission to a hospital equipped with a Drinker or Bragg-Paul apparatus can be obtained. In severe cases of generalized paresis, in which various groups of skeletal muscles are involved, massage, passive movements, and electrical treatment are necessary. Tonsillitis, adenitis, and otitis media, occurring in the course of convalescence, are treated on ordinary lines.

ISOLATION

Diphtheria patients are usually kept in isolation until two or three consecutive negative swabs from nose and throat have been obtained at weekly intervals.

[For details regarding the precautions to be observed during isolation, intravenous serum administration, and the treatment of serum sickness, attention is invited to the corresponding section of the article on scarlet fever.]

Laryngeal Diphtheria

Whilst important developments have taken place in recent years in the diagnosis and treatment of laryngeal diphtheria as a result of the application of endoscopic methods by which the larynx may be inspected, membrane removed by aspiration from the cords and subglottic space, and direct intubation performed through the laryngoscope, these remain entirely in the realm of hospital practice. Short of these methods operative intervention consists in indirect intubation and tracheotomy. In indirect intubation a cannula is introduced through the mouth into the larynx by means of the special introducer devised by O'Dwyer, but this is also very much a hospital procedure, requiring specialized medical and nursing care. For the practitioner, therefore, the main essentials are specific treatment, expectant treatment in cases in which signs of laryngeal obstruction are not urgent, and the operation of tracheotomy for the relief of urgent symptoms. Specific treatment has already been dealt with.

Expectant Treatment

As soon as a diagnosis has been made diphtheria antitoxin should be given. The question of operation will occur to the practitioner, but if signs and symptoms indicate the initial stage of laryngeal obstruction—that is, are limited to hoarseness, quickened respirations, a characteristic croupy cough, but no dyspnoea—expectant treatment should be adopted. The patient should be

placed in a half tent, improvised by placing an ordinary draught screen round the head of the bed and roofing it over with a sheet for about one-third of its height from the top. Into this tent is led steam from a bronchitis kettle, over the spout of which are tied a few thicknesses of gauze soaked in tinct. benzoin. co. Frequent hot fomentations are applied to the neck, and I employ the following mixture for its antispasmodic effect in full doses (1 drachm to 1½ drachms) four-hourly, according to age:

℞ Tinct. belladonna ... ... ♏ iv
Tinct. camph. co. ... ... ♏ vij
Aq. ... ... ad ʒ j

The onset of a sudden emergency, such as the occlusion of the airway by detachment of a piece of membrane, is always a possibility, and the practitioner should visit as frequently as possible to see how the patient is responding to treatment, and should arrange to be within easy call.

Tracheotomy

If when first seen the child is obviously in the spasmodic stage, or in spite of expectant treatment this stage has been arrived at, as shown by stridor, aphonia, restlessness, cyanosis, spasms of coughing, dyspnoea, and well-marked recession of the soft parts in the lower intercostal spaces, the pit of the stomach, and at the root of the neck, then operation should be decided on at once. In hospital it is permissible to hold on to expectant treatment further, but for the general practitioner the situation is well summed up by Ker, who suggests that in the above circumstances there are three alternatives: "The first is to send the child into hospital at once, the second is to live in the house and not leave the child for a moment, and the third, which is sometimes the only one possible for him to adopt, is to perform tracheotomy on the spot."

When the operation is performed in private practice a general anaesthetic, such as chloroform, may be given, but if time permits a local anaesthetic is to be preferred. Frequently the urgency is so great that the operation will have to be done without anaesthesia. The patient is wrapped tightly in a blanket to control the arms and legs, and placed on a table with a rolled-up sheet under the shoulders. The head must be held by an assistant so that the median plane is exactly at right angles to the floor, and this position must be maintained till the trachea is incised. Before commencing operation it is a wise precaution on the part of the operator, if he does not usually wear spectacles, to provide himself with a cheap pair of plain glasses to protect his eyes.

The skin having been prepared the practitioner should make sure of the cricoid cartilage, placing his second finger and thumb on each side of the larynx and the index finger on the middle of the lower border of the cricoid. Throughout the operation the left hand should not be moved from this position. The incision is then made downwards from the lower border of the cricoid for about an inch, commencing at the tip of the left index finger and taking care that the incision does not tail off to one side or the other. Dissection should then be made through the various tissues until the rings of the trachea are recognized, cutting through the isthmus of the thyroid gland if necessary. At this stage much venous oozing will probably occur, but this should be met by swabbing rather than by the application of forceps. When the trachea is reached the upper two rings should be severed and laryngeal dilators inserted into the wound. There will be immediate expulsion of blood-stained frothy mucus, and the patient should be allowed to cough the upper air passages clear before the tracheotomy tube is put into place. To prevent the aspiration of blood at this stage, it is good practice to elevate the trunk above the level of the head and neck. A suitably sized Parker's tube, with a fenestra in the shoulder, is then inserted (one fitting fairly closely to the trachea is to be preferred) and tied in position with a layer of boric lint between the shield and the skin: the patient is returned to the steam tent for twenty-four hours after operation.

After-treatment

While the immediate results of the operation are frequently spectacular, the remote results, which ultimately depend on the successful removal of the tube, must be considered. The after-care consists in the careful supervision of the patient and attention to the inner tube, which must be removed every few hours and thoroughly cleaned out in the following solution:

℞ Acid. boric. ... ... ʒ ss
Sod. bicarb. ... ... ʒ ss
Aq. ... ... ad O j

The outer tube should not be removed except in an emergency, but if a sudden obstructive attack occurs it must be withdrawn and laryngeal forceps inserted in an attempt to remove the obstructing membrane. If mucus tends to become inspissated cautious spraying of the trachea with sod. bicarb., 15 grains to the ounce, may help to loosen it. An expectorant mixture containing ammonium carbonate may also be prescribed. During fits of coughing, also, the tube may be displaced from the trachea, the signs being the return of dyspnoea, stridor, and recession.

When the airway is well established and the respiration comfortable, the patient should be kept as quiet as possible for three or four days. During this time he is spoon-fed. On the third or fourth day the plugging of the outlet of the tube with the finger should be attempted for a few minutes to see if respiration can occur by way of the fenestra and the natural passage. If the patient is comfortable while this is being done the tube should be quietly removed, and if there is no return of dyspnoea for an hour or two after removal it is unlikely that reinsertion will be required. Should signs of respiratory obstruction follow the withdrawal of the tube it should be replaced, but daily attempts must be made to get it out. In some children the main obstacle to this is psychological, but organic obstruction due to granulation above or below the wound may be the cause. In the latter case the introduction of a larger tube may remove the difficulty, but when the granulations are above the wound delay in seeking the expert aid of the laryngologist should not be incurred lest complete laryngeal stenosis result.

This unfortunate sequel, in my experience, has usually been the result of damage caused to the larynx by an inexperienced, agitated operator, but it may also follow the most perfectly executed operation, and it would be wise to call for direct laryngoscopy if at the end of two or three weeks it is impossible to get the tube out. Bronchopneumonia may also follow tracheotomy within a day or two of operation, and should be treated on general lines by stimulants, poulticing, oxygen, and abundant nourishment, but the outlook in this complication is very grave.

Treatment of Carriers

An individual should not be designated a carrier until the diphtheria bacillus obtained on culture has been shown to be virulent. The carrier state may be related to the throat, nose, both throat and nose, and, infrequently, the ear, and is perpetuated by some pathological or anatomical abnormality in the region affected. Efforts must be directed to the eradication of this, and the carrier's general condition should be toned up with nourishing food and exercise in the fresh air. Insufflations of dimol directed into the nose and throat by means of an atomizer are helpful, and if the patient will co-operate, douching with a solution of equal parts of sodium bicarbonate, sodium biborate, and sodium chloride, 2 drachms to the pint, should be carried out three times

daily, using half a pint at each douching, as recommended by F. H. Thomson, E. J. Mann, and H. Marriner.

Should these measures fail after three or four weeks' conscientious trial, the nasopharynx should be thoroughly examined by a throat specialist with a view to operative treatment. In throat carriers tonsillectomy can be relied on to terminate the carrier state in practically every case. Nasal carriers should be examined with a view to clearing out such defects as hypertrophied adenoids, enlarged turbinals, or deflected septa. Ear cases, in which the diphtheria bacillus is usually associated with a chronic otitis media, may require a mastoid operation. Proof of the termination of the carrier state should only be accepted when the patient has given three consecutive negative cultures at weekly intervals.

TESTS FOR RESPIRATORY EFFICIENCY

A CRITICAL REVIEW

BY

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Ever since the researches of Hutchinson on vital capacity in the middle of the last century many workers have considered methods which might be of help in assessing respiratory efficiency. Dr. Alan Moncrieff, in a recent valuable monograph,[1] gives an admirable survey of the tests which have been recommended in the past. After careful consideration of them all he selects four of these as giving the most useful and reliable information, and describes the results of applying these tests in a group of eighty-six patients suffering from various pulmonary affections. The tests employed were the measurement of the vital capacity, the determination of the ventilation equivalent for oxygen, the measurement of the dead space, two tests with the mercury tube (the maximum height to which the mercury column can be raised by an expiratory effort and the time for which the mercury can be maintained at a level of 40 mm.), and the ratio of expiratory to inspiratory time as shown by graphic records. A careful and detailed account is given of the way in which the tests are best carried out, and it is explained how they can be completed in less than ten minutes. Dr. Moncrieff has classified his cases according to the clinical estimate of their respiratory state into "good," "fairly good," "fair," "poor," and "bad," and has attempted to correlate the clinical assessment with the results obtained from the use of the tests that have been enumerated.

VITAL CAPACITY

The vital capacity, as is well known, depends on the age, sex, height, weight, and the physical fitness and occupation of the subject. Normal standards have been worked out with which the figures obtained in patients can be compared. It may be reduced by 20 to 30 per cent. by physical weakness uncomplicated by organic disease. Moncrieff's studies show that on an average there is a fairly satisfactory relation between the clinical estimate of respiratory inefficiency and the degree of reduction of the vital capacity. The averages for the cases labelled "bad," "poor," and "fair" were 36, 54, and 69 respectively (with 100 taken as the normal standard), against 76 and 97 for the groups described as "fairly good" and "good." In the "poor" group all the cases had a vital capacity below 74, but in the "fairly good" nineteen out of thirty-one, and in the "fair" seven out of twenty-nine, had a capacity of 75 or over. It seems, then, that significance can only be attached to a low figure for the vital capacity. In none of the cases described as "good" did the vital capacity fall below 70. A case is, however, quoted which serves as a warning of the limited value of an isolated test—that of a man whose clinical state was "fair" and who suffered from a pneumothorax. His vital capacity was well above the normal.

VENTILATION EQUIVALENTS

The term "ventilation equivalent for oxygen" was introduced by Anthony in 1930, and is defined as the volume of air inspired (in litres) per 100 c.cm. of oxygen consumed; in the normal subject its value is on an average 2.5 litres. By expressing the pulmonary ventilation in this fashion variations due to differences in metabolic rate and consequently in oxygen consumption are excluded. The ventilation equivalent and the respiratory exchanges can be readily determined by means of a modified Knipping apparatus, and convincing evidence is provided that this method is reliable; it has the advantage over the usual Benedict-Roth type of apparatus that the carbon dioxide output as well as the oxygen consumption can be measured.

The ventilation equivalents in Moncrieff's series were quite irregular in their distribution both in respect of the average in the different groups and the range within each group. This is not altogether surprising when one bears in mind that the ventilation equivalent is, as explained, simply a measure of the amount of respiratory effort involved in obtaining a standard volume of oxygen. It must be remembered that breathing serves other purposes than the provision of oxygen. It is affected, for example, by alterations in blood reaction; thus in states of acidaemia (such as occur in diabetes or nephritis) the pulmonary ventilation is increased not to provide oxygen but to eliminate excessive amounts of CO_2 in order to preserve the normal alkalinity of the blood. Though the ventilation equivalent would be high in such conditions it does not indicate respiratory insufficiency, but merely means that breathing is being used for a purpose other than the provision of oxygen. The hyperpnoea produced by emotional causes would result in a similar raising of the ventilation equivalent, but would throw no light, of course, on the state of the respiratory apparatus. It is likely that such emotional hyperpnoea occurs very commonly in patients who are being examined with novel apparatus. On the other hand, in states of central respiratory depression such as that produced by morphine poisoning the ventilation equivalent would be low. That merely means that breathing is becoming inadequate for its purpose rather than indicating a high level of respiratory efficiency.

SIZE OF DEAD SPACE

The size of the dead space is calculated from a knowledge of the composition of the expired and the alveolar air and the measurement of the tidal air, the formula employed being:

$$\text{Dead space} = \frac{\text{Tidal air } (CO_2\% \text{ in alveolar air} - CO_2\% \text{ in expired air})}{CO_2\% \text{ in alveolar air.}}$$

It must be acknowledged that alveolar air determinations are difficult to carry out accurately in untrained subjects. In all probability the anatomical dead space—or in other words the actual capacity of the respiratory passages—alters comparatively little in different conditions, and is only affected by constriction or dilatation of the bronchioles. Calculated in the manner described above, the dead space usually amounts to about 150 c.cm. and constitutes 25 to 30 per cent. of the tidal air. In some conditions the apparent or calculated dead space rises to 500 or 600 c.cm., and may constitute 60 or 70 per cent. of the tidal air. These figures cannot represent a true anatomical change, but indicate that a larger proportion of the tidal air is functioning as dead space in the sense that it is not helping to remove carbon dioxide from the alveoli of the lungs. Moncrieff very properly suggests that the dead space should be expressed not as a volume but as a percentage of the tidal air, and put in this way it indicates what proportion of each respiratory effort is being turned to good use in eliminating CO_2. Dead space determinations carried out in this way are influenced very readily by emotional hyperpnoea, and this probably reduces the value of the test in untrained subjects. On the whole, Moncrieff's results show that there is little correlation between the size of the dead space and the clinical state of the patient. In all probability this is because the factors which have been

[1] *Tests for Respiratory Efficiency.* By A. A. Moncrieff. Medical Research Council Special Report Series, No. 198. H.M. Stationery Office. (1s.)

mentioned as modifying the ventilation equivalent similarly influence the determination of the dead space.

The average results with both types of mercury U tube tests in Moncrieff's series fall well below the standard minima required by the Air Force regulations; these latter are probably too high for the hospital population, but the results obtained are shown to fall below the average for hospital out-patients not suffering from cardio-respiratory disease. Unfortunately, there were a considerable number of individuals in each group who reached or attained the normal standards, and this limits the value of the test in practice.

The expiratory to inspiratory ratio obviously throws light on cases of asthma, and it is further claimed by some workers that what they call the respiratory style of the individual may bear some relation to the ease with which dyspnoea can be induced.

Complexity of the Problem

Moncrieff's historical review and the results which he has obtained prove very clearly that tests for respiratory efficiency present a more complex and less readily solved problem than that which has to be faced when one attempts to assess cardiac or renal function. The heart and kidney are, after all, merely organs, while breathing is a function which depends for its effective execution on the nicely co-ordinated activity of the brain, spinal cord, and peripheral nerves, various groups of muscles, the respiratory passages, and the lung substance. It has to perform a number of tasks, such as the provision of oxygen, the elimination of carbon dioxide, the regulation of blood reaction, and the maintenance of normal temperature; and it is influenced by numerous nervous and humoral factors. The question is further complicated by the fact that a good deal of confusion appears to exist as to what exactly is meant by the term "efficiency" as applied to respiration.

When we talk of the mechanical efficiency of a muscle we refer to the degree of economy of the process in the sense of how large a proportion of the total energy of the chemical reactions taking place during contraction is converted into mechanical work. This sort of idea seems to be behind the ventilation equivalent and the dead space tests. But it has already been pointed out in respect of the ventilation equivalent that it is only a measure of the economy of breathing in relation to one objective—namely, oxygen consumption. The breathing may be extravagantly large for this purpose, but still be well adapted for serving some equally urgent need such as the regulation of blood reaction.

The dead space determination measures two forms of efficiency: first, the proportion of the air inhaled which is used for alveolar ventilation; and, secondly, the efficiency with which CO_2 is being eliminated. Ventilation may in certain circumstances appear excessive and wasted from the point of view of CO_2 elimination and yet be just adequate for other purposes, as, for instance, in states of anoxia where the overruling aim is to supply sufficient oxygen. It has already been sufficiently stressed that both the ventilation equivalent and the dead space measurements are frequently vitiated by the presence of hyperpnoea resulting from the emotional excitement which accompanies the test, and the determinations may frequently not be indicative of the normal style of breathing. The other sense in which respiratory efficiency might be used is as a measure of the ability of the breathing apparatus to carry out its functions satisfactorily under conditions of increasing strain. When we test cardiac efficiency we see how much work an individual can do in a given time without the development of pain or undue respiratory distress. Similarly in the case of renal tests we study the power of eliminating large quantities of water or waste products and the degree to which the normal composition of the blood can be maintained.

Looking at breathing from this point of view a new series of possible tests present themselves which appear to have a sound physiological basis. One could determine, for instance, the degree of increase in the ventilation in response to excess of CO_2 in the inspired air; the degree to which the alveolar CO_2 level is kept constant in such circumstances; the response to varying grades of oxygen lack; the response to acidaemia or alkalaemia produced, for example, by the ingestion of ammonium chloride or of sodium bicarbonate. A great deal of information is already available about the normal reactions to such tests, and their study in respiratory disease might be of considerable value, and seems to present a promising field of investigation. It must be confessed, however, that such tests are very elaborate, and would involve the expenditure of considerable time and effort. For general clinical use much simpler tests would be desirable, but Moncrieff's review clearly shows that on the whole the tests that have been devised in the past give information of only limited value.

When Tests are Essential

The whole question is still further complicated by the fact that breathing is nicely co-ordinated with the circulation so as to serve the purpose of effecting satisfactorily gaseous interchanges between the outside air and the tissues. In examining the response to exercise both the circulatory and the respiratory systems are being tested. It is probably fair to say that in the absence of disease of other organs or systems dyspnoea occurring too early, or limitation in the field of response, would be indicative of insufficiency of the respiratory apparatus. It is possible that the usual tests of cardiac efficiency could be modified to throw light on respiratory function. It must be recognized, however, that it is only in a limited number of cases that one would require objective tests of this character to aid one in assessing the present condition and future progress of an individual patient. A routine clinical and radiological investigation and a wide experience would, in the majority of instances, give all the information required. There is, however, a group of cases in which further help is urgently needed, and one has in mind especially patients who are to be subjected to surgical procedures such as thoracoplasty. In these one ought really to consider the various stresses to which they are going to be subjected during the course of the operation and to try to determine in advance, as far as possible, what effect they are likely to have on the state of the patient. It is more a case of assessing the patient's ability to withstand a series of shocks than of determining his respiratory efficiency in either of the ways in which it has been defined.

A. I. Goldschlager (*Arch. Int. Med.*, February, 1935, p. 939), who records eight cases, states that trichinosis occurs more commonly than is generally known, as is shown by the following figures: 263 cases from fifteen States were reported by the United States Public Health Service in 1932, and 208 cases from thirteen States in the first nine months of 1933, more being reported by New York and California than by any other State. All Goldschlager's cases, which occurred in patients aged from 16 to 60, were due to consumption of imperfectly cooked pork or sausages. Oedema of the eyelids, intense headache, and pain on flexing or extending the neck were the outstanding features of all the cases. The intradermal tests introduced by Bachmann in 1928 were positive in all cases in 1 in 100, 1 in 1,000, and 1 in 10,000 dilutions. The results of precipitation tests were less positive. It is stated that all the patients recovered. Mild trichinosis may very easily be overlooked or mistaken for influenza or other infectious diseases.

TO MELBOURNE AND BACK

PART IV.—THE CORAL SEA, JAVA, MALAYA

The evening of Saturday, September 14th, will be the last in Melbourne, but final leave of friends and hosts will not be taken until 9 a.m. on Sunday, when the homeward voyage in the s.s. *Nieuw Zeeland* of the Java-Australia Line will begin, a voyage which will be no less packed with interest than was the outward one. By dawn on Tuesday the distance to Sydney will have been retraced, and a stay of two days here will permit a closer acquaintance with this city and its beautiful surroundings than was possible during the previous brief glimpse. Most of the members of the party will already know how they intend to pass the time at Sydney, and the stay will not seem any too long.

Two more days' steaming will bring them to Brisbane, but only a few hours will be available for Queensland's capital, which is charmingly set in the bend of the Brisbane River eighteen miles from its mouth. It is a city of cathedrals and impressive State buildings, and its outer residential districts are really beautiful, with their well-designed houses standing in gardens of rich colouring, where the scarlet poinsettia sets a dominant note.

THE FORMATION OF THE CORAL REEF

The next stage of the voyage, beginning on Saturday afternoon, will be along the Great Barrier Coral Reef, which stretches for 1,200 miles by the north-east coast of Queensland. The Reef is a colossal coral rampart, from 10 to 150 miles wide, built up from the floor of the ocean by the microscopic coral polyp, in its efforts through the centuries, as the ocean bed subsides, to keep its home at the level of the warmer waters. The result of this ceaseless constructive activity is a basic wall of limestone studded with coral atolls and sand cays, where floating vegetation, sand, and mud, caught and held by the myriad arms and recesses of the coral formations, have gradually consolidated and formed little islands above the surface of the sea. Where no islands have formed the coral grows in what is to all appearances a chain of gardens. Trees, shrubs, and flowers of living coral, in fantastic shapes and unbelievable combinations of soft colouring, grow in endless profusion, and when the tide is low parts of this extraordinary submarine forest stand exposed to view. At high tide the submerged coral gardens are explored in glass-bottomed boats, and are seen to be alive with vividly coloured fish, which dart in and out of the coral foliage, while shellfish and weird sea animals lie or swim about the bottom of each pool.

Voyaging along the north-east fringe of the Great Barrier Reef, and south of New Guinea, the ship reaches Macassar, the seaport of Celebes, on Sunday, September 29th. This is the travellers' introduction to the Dutch East Indies, where they will strike a new, rich note of colour, different from the delicate hues of the coral forest, and equally fascinating. Here everything living, whether plant, animal, or human being, contributes its quota of brightness to the scheme. Birds and flowers glow with colour, the natives dress in vivid hues, and each evening the blazing tints splashed across the western sky by the passing day seem to set a final seal on this brilliance.

BY ROAD THROUGH JAVA

At daylight on Tuesday, October 1st, Sourabaya is reached, and instead of remaining aboard for the five-day stretch between Sourabaya and Batavia passengers can make the journey overland by car through the heart of Java. In this island climatic conditions and a volcanic soil rich in phosphoric acid combine with a most thorough irrigation system to produce vegetable life in great profusion. This is especially noticeable in the Botanical Gardens at Buitenzorg, where trees blossom rather than plants, and flowers seem to run riot, so that gardening is more a matter of restraining growth than of developing it.

The motor route is through Djocja, near which is the remarkable Buddhist temple of Borobudur. This is seven stories high, was built by priests eleven hundred years ago, and contains three miles of colonnades, with over 5,000 stone carvings depicting the life of Buddha. When the Mohammedans conquered Java the priests of Borobudur, to save their temple, covered the whole of the huge building with earth and planted trees on it. The fertile Java soil soon coated the mound with thick vegetation, and for 600 years the temple was lost. In 1814 Sir Thomas Stamford Raffles, then Governor of British Java, caused it to be unearthed, since when the Dutch have completed its restoration with an admirable exactness.

Visits will be made to the Prambenan Temples and Wanosobo, and then the party will drive via the Serajo Valley to Garoet, set among the volcanic mountains 2,400 feet above the sea. Here the terraced rice fields on the steep hillsides, with water running from terrace to terrace, are an object lesson in simple cultivation. From Garoet the journey will be via Bandoeng and Buitenzorg, through more rice fields and plantations of tea, rubber, and cinchona, to Batavia, the capital of the Netherlands Indies. This town is divided into an ancient and a modern quarter, the one differing vastly from the other. Old Batavia, with its seventeenth century Dutch houses along the canals, is now occupied only by Chinese and Malays. Batavia Centrum, the present residential quarter, was laid out about a century ago, and is a modern and up-to-date district, with recreation grounds, good hotels, cinemas, and a racecourse.

TWO FAMOUS PORTS

The voyage by boat will be resumed on Sunday, October 6th, at about four o'clock in the afternoon, and after thirty-six hours the seaport of Singapore will be reached. This island, the transhipment centre of the Orient, stands to-day as a monument and tribute to the far-sighted genius and courage of Stamford Raffles, who established a trading station here in 1819, and as long ago as 1824 predicted that Singapore would become "the emporium and pride of the East." How fully Raffles's judgement has been vindicated is well known, and the travellers will look with keen interest at the fine harbour, with its great floating dock, the busy wharves, where a wonderful variety of vessels are always to be seen, the well-planned modern section of the town, and the native quarter. Here the Buddhist and Hindu temples and Mohammedan mosques are eloquent of the cosmopolitan range of Singapore's half-million inhabitants.

Late in the evening of Wednesday, October 9th, a special train will take the party to Kuala Lumpur, the capital of the Federated Malay States. A stay of two days will be available in which to explore this very picturesque town and its surroundings. Visits will be made to rubber estates, tin mines, the Batu Caves, the Botanical Gardens, and the Museum. Something will be learned of the remarkable achievements of medical science in British Malaya, and golfers will have a chance of a round on the finest golf course in the East. At about 8 p.m. on Friday the train leaves Kuala Lumpur for Penang, which was founded by Captain Francis Light in 1786, and is second only to Singapore in importance as a Malayan port. Here visitors may be able to drive round the island, see the famous Waterfall Gardens and the Snake and Ayer Itam temples, and, if time allows, they may take the cable tramway up the Crag, and enjoy the panorama of Penang from a height of 2,000 feet. In a few hours the P. & O. liner *Rajputana* will await their embarkation in the harbour, when the journey will be resumed to Ceylon.

The Institute of Medical Psychology (Malet Place, W.C.) announces that twenty lectures on psycho-physical adaptation, being an introductory course of psychological medicine for the general practitioner, will be given on Wednesdays, from April 24th to June 26th, by Dr. H. Crichton-Miller (3 p.m. to 4 p.m.) and by Dr. Cedric Shaw (4.30 p.m. to 5.30 p.m.). The fees are: for either series, £1 11s. 6d.; for both series, £2 2s. Tickets must be obtained in advance from the educational secretary at the Institute.

OSTEOPATHS BILL: SELECT COMMITTEE

EIGHTH SITTING

Evidence of the British Medical Association

The House of Lords Select Committee on the Registration and Regulation of Osteopaths Bill sat again in the King's Robing Room on March 29th, when the hearing of the applicants against the Bill was continued. Lord Amulree presided.

Sir William Jowitt, K.C., in opening the case for the British Medical Association, said that the Association approached the matter solely in the public interest. If the public interest conflicted with that of the medical profession, the Association would be the first to say that its own claims must be brushed aside. In this country at the present time there were 2,000—some estimates said 3,000—osteopaths who were unqualified, and 179 who boasted some qualification. The Bill was brought forward on the hypothesis that the unqualified were doing some public harm. The 179 were made up, as to ninety-six, of those who had gone through the British School of Osteopathy at Buckingham Gate, and as to eighty-three, of those who had some American qualification. The proposal in the Bill was that all persons practising osteopathy should come on to a register, admission thereto being subject to the conditions laid down in Clause 6. It was for their lordships to consider whether the persons practising osteopathy to-day should be required to submit themselves to an examination. The comparatively recent precedent of the dentists had been cited; there State registration had taken in a large number of persons whose entitlement was solely on the ground of previous practice. The reasons which compelled this course in the case of the dentists, and also in that of the veterinary surgeons, were the insufficiency of qualified persons and the certainty that once the register was established all fresh entrants into the profession would be adequately qualified, and to-day no one could practise dentistry without having gone through the curriculum and obtained registration. The present Bill proposed to establish a register of osteopaths to include a very large number of people who had no qualification beyond the fact of having practised, and many of whom, as the promoters of the Bill averred, had practised in such a way as to constitute some public danger. This might be accepted if it involved the corresponding advantage that hereafter all who practised as osteopaths would be properly trained and qualified. It was here that the case for the Bill broke down.

Where Would the Registered Osteopath be Trained?

If the osteopaths, following the advice of Mr. Neville Chamberlain when Minister of Health, had first established a proper school, with an attached hospital, they would have had a case which might well have inclined their lordships' House to consent to the little wrong of placing unqualified people on a register in order to achieve the greater good that thereafter there would be no unqualified persons. But Dr. Kelman Macdonald—to whom he wished to pay a high tribute, both for his personality and for the manner in which he had given evidence—had said that there was no reputable school except the British School of Osteopathy. He (Sir William Jowitt) did not want to say very much about that school or its principal, Dr. Littlejohn. The divergence between Dr. Macdonald and Dr. Littlejohn was as great as between the North Pole and the South. The British School of Osteopathy could not be regarded as in any way a satisfactory or, he regretted to say, a reputable school. Of its ninety-six "old boys" some fifteen were Looker students, and he had put to Dr. Littlejohn the case of one who got a certificate saying he had attended the four-years course at the school when in fact he had attended the school for one year. "That is not an honest certificate, and Dr. Littlejohn, I say quite plainly, is not an honest man. Anybody who has the interests of osteopathy at heart would regret to see that a man of the quality and calibre of Dr. Macdonald is in any sense mixed up with a man of the quality and calibre of Dr. Littlejohn."

If there were only more people like Dr. Macdonald—but one swallow did not make a summer—who could give their time and attention to the development of the school and its attached hospital, he could conceive that their lordships would approach this Bill from a different point of view. It was very significant that Dr. Macdonald and his association had never recognized the students of the British School of Osteopathy. In these circumstances to ask their lordships to say that the State ought to recognize osteopaths, although the only prospect of the osteopaths having any education was in the British School, was to ask them to do something which was quite impossible.

Dr. Macdonald, with the candour and intelligence which he had displayed throughout, had admitted that State registration would give a certain *cachet*. On what grounds could osteopaths claim this distinction? They were asking to be placed on a State register as qualified to treat all diseases, yet no evidence had been brought forward that osteopaths treated acute diseases. Dr. Macdonald and Mr. Streeter had "office clinics" which offered no facilities for the treatment of acute cases. Again, the fundamental reason why the doctor was better able to treat disease than a layman like himself was because of his ability in diagnosis, but there was not a tittle of evidence to justify the State in placing osteopaths on a register as persons fit and qualified to treat, for example, pneumonia. The extent of the claim was impossible. There was no evidence that any osteopath in this country was treating acute disease. Indeed, why, if an osteopath was a fit and proper person to treat pneumonia or meningitis, were these limitations put in the Bill, that he must not sign a death certificate, that he must not give an anaesthetic, that he must not deal with obstetrics? An attempt was made to distinguish between major and minor surgery, but this was quite impossible, as was shown by the different decisions in different localities as to whether or not a certain surgical service—enucleation of the tonsils, for example—came within a general practitioner's obligation under the Insurance Act. As for drugs, Dr. Macdonald had himself admitted that it would be necessary to teach the young osteopath their use, and in many of the acute diseases, which osteopaths asked that they might be recognized as able to treat, it was agreed that certain specific drugs were necessary.

Freedom of Opinion

Sir William Jowitt went on to say that in his view education was only secondarily the acquisition of knowledge; it was primarily the evocation of the student's ability to think for himself. He would deplore a sectarian education. It was idle for a person like himself to try to distinguish between the osteopathic theory as advanced by Dr. Macdonald and the medical theory as advanced by Sir Farquhar Buzzard, but he was sure that the proper method was to give to the student intending a professional career a liberal education, unprejudiced and uncommitted to a particular point of view. It would be lamentable to turn out into the world persons who, on entering the practice of the healing art, were already pledged, at a time before they could truly know the facts, to a particular theory or school. The medical profession might have been slow to take up the practice of manipulation, but to-day in medicine itself a great deal of thought and training was devoted to this subject, and if there was anything in the manipulative theory it seemed to him that it might well be allowed to follow its natural evolution. The coming doctor would not disregard any value it might possess, for it would engage his professional reputation and financial interest. If the osteopathic concept was right, why did not the young doctors coming from the medical schools accept it? They were in a position to judge, their minds were unfettered, and they could pick out from the theory what was good and reject the dross.

In conclusion, counsel asked whether it was too much to hope that the right solution might be found in letting those who would in future practise osteopathy first of all take their medical degrees. Dr. Kelman Macdonald himself was a striking example in point. Within the profession they would be perfectly entitled to carry on their researches in any way they liked in the interests of their patients, and the British Medical Association would not countenance any ostracism of those who followed a particular theory and practice.

Sir Henry Brackenbury's Evidence

Sir Henry Brackenbury, Vice-President and former Chairman of Council and Chairman of Representative Body of the British Medical Association, one of the direct representatives of the profession on the General Medical Council, past-president of the Association of Education Committees of England and Wales, then gave evidence. He was first asked by Sir William Jowitt to describe the organization and aims of the British Medical Association. He said that the Association was a purely voluntary organization of about 35,000 members, which had been recognized for many years by Governments and local authorities as representing the whole profession. It included in its membership not only a large majority of the general practitioners of the country, but specialists and consultants, teachers and research workers, and public health officers of all kinds. Its functions fell under four main headings. The most important was scientific and educational; the Association spent large sums of money every year under this head. Next came the medico-sociological work, which was very extensive. The Association also tried to promote a high ethical standard of conduct amongst its members, but professional discipline was a matter which concerned the General Medical Council. Finally, there was the economic aspect. The Association endeavoured to safeguard the status, remuneration, and conditions of employment of those members of the profession who were employed in some public capacity, but that occupied a relatively small place in its entire work. The Association approached the present issue from the public point of view, realizing that any sectional interest must give way to public welfare.

The Medical Curriculum

The witness next outlined the medical curriculum. There was, he said, a movement by the General Medical Council to place the whole of biology in the pre-medical stage, instead of partly in the pre-medical and partly in the medical, so that the actual five years of medical education would begin after physics, chemistry, and biology had been completed. Five years was the bare irreducible minimum after the pre-medical. If the whole of biology were taken in the pre-medical, the start of the five years proper would be somewhat postponed. From first to last the average time spent on the curriculum would be about seven years. After qualification the young practitioner still went on learning, otherwise he would be not only a scientific but an economic failure. He was at liberty to adopt any theory of medicine and surgery. A fundamental restriction upon the powers of the General Medical Council prevented it from erasing any name from the *Register* on the ground of such adoption. Dr. Macdonald was at liberty to practise osteopathy or anything he liked, and any practitioner could consult with him.

Lord Elibank: Dr. Macdonald indicated to us that there was some difficulty in that respect.—There is this perhaps to be said. Dr. Macdonald being on the *Medical Register*, there could be no question about the propriety of any man consulting with him if he wished to do so, but at the same time if I wish to consult with a colleague the value of that consultation is vitiated if I know beforehand that he has adopted a fixed and restricted method of looking at things. In such a case I might hesitate to consult him because I might think such consultation would be unprofitable on account of the fundamental difference between the way he and I looked at things. That depends, of course, upon the nature of the case under consultation.

But Dr. Macdonald said there was direct ostracism because he was practising as an osteopath.—I understood Dr. Macdonald to indicate that in two separate respects. My only information about the conduct of the medical profession in Edinburgh in relation to Dr. Macdonald does not bear that out in the least. I am assured that there is very considerable freedom of consultation with Dr. Macdonald in Edinburgh, because, of course, he is a person on the *Register*, and also because his personality is respected. It would be wrong to assume that there is any ostracism of Dr. Macdonald by the medical profession.

He told us he was not allowed to follow his patient into a fever hospital.—That would apply to all of us. We could not as a matter of routine follow our cases into an infectious diseases hospital, or into most other hospitals. I am myself on friendly terms with the medical superintendent of the infectious diseases hospital in my own area, and I was for many years a member of the local authority which owns the hospital, yet in neither of those capacities have I any right to follow my patient into the wards.

Would you yourself deprecate the ostracism of an osteopath merely because he was an osteopath?—Certainly.

Have you yourself ever used or invited the co-operation of an osteopath?—Probably in the sense in which you mean it, no. I have had a number of patients who have expressed a desire to see an osteopath about certain things, and I have never placed the slightest obstacle in their way.

You have not advised them not to go?—Never.

Do you recognize that there is something in osteopathy?—If I may diagnose and choose my cases first, absolutely.

Sir W. Jowitt: Dr. Macdonald also mentioned a case in which a bacteriologist refused to prepare for him an autogenous vaccine.—That was very wrong. I think that Dr. Macdonald was treated badly and in a way he should not have been. But to take that as a general example would be improper.

The Ethics of Co-operation

The Chairman asked whether the medical man who refused to assist Dr. Macdonald could be brought before the General Medical Council for unethical conduct, to which the witness replied that that question did not arise. This was the question of an action of one individual towards another, and he did not think it was a case for judgement by a tribunal. It was the opposite question which came before the General Medical Council: If he had co-operated would he have been guilty of unethical conduct? to which the answer was "No." But no medical man could be compelled to consult with another. He thought the British Medical Association would say that in refusing to perform this service for Dr. Macdonald the medical man had acted improperly, and that he ought to have co-operated. Lord Elibank thereupon asked why the famous case of Dr. Axham was allowed to occur, to which the witness replied that that related to co-operation with an unregistered person; Dr. Macdonald was a registered medical practitioner.

Therefore, unless he is a registered medical practitioner as well as an osteopath the British Medical Association or the General Medical Council would not countenance co-operation with an osteopath?—The British Medical Association has nothing to do with that; the General Medical Council has, and what the General Medical Council is anxious to do so far as I can speak for it—I am a member, but my membership is of later date than Dr. Axham's case—is to prevent "covering." There are no fewer than forty Acts of Parliament which impose duties upon the registered medical practitioner and do not bind anybody else. The professional offence to which the General Medical Council directs its attention is "covering"—that is, enabling an unregistered person to practise as if he were registered. If habitually a registered medical practitioner helps an unqualified and unregistered practitioner to carry on his practice, just coming in to help him in those things, such as death certification, which the State says must only be done by a registered medical practitioner, then that practitioner is liable to be called to account.

The witness added that the General Medical Council did not go out heresy hunting. It had no power to commence proceedings in such cases on its own initiative; a complaint must be lodged. Mention had been made of osteopaths who found difficulty in getting a general practitioner to give an anaesthetic for them. If a general practitioner did so, the General Medical Council could take no cognizance of it; it could only act on a complaint received.

Sir William Jowitt said that if such a case did come before the Council the witness would be one of those required to act in a judicial capacity, and therefore he would ask him no further questions on that subject.

The General Character of Medical Practice

In further reply to counsel, Sir Henry Brackenbury said that three-quarters or so of medical students who passed the examinations entered upon general medical practice; the other quarter either went into the public health service, or became consultants or specialists of some kind, or—a relatively small proportion—adopted a teaching or research career. There was a large body of medical work which was common to all members of the profession, a larger body still common to all general practitioners, but the exact sphere and content of a man's work could not be defined; it depended partly upon the locality and partly upon his own inclination

and special skill. He mentioned colliery practice, where a general practitioner's manipulative skill had its greatest opportunity.

Lord Elibank: You say there are a number of general practitioners who practise manipulative surgery in colliery districts. Where do they learn?—Like others they have learned at their medical schools, but of course their skill in their particular branch develops with experience in practice.

Is it part of the curriculum of a medical school?—Yes, it is compulsory. It was emphasized very much when the General Medical Council made its last revision of the medical curriculum in 1926.

Sir Henry Brackenbury was next asked with regard to national health insurance experience so far as concerned a possible differentiation between major and minor surgery. He expressed the view that it would be impossible—or at least subject to continually conflicting decisions—to institute a restrictive measure whereby a person would be allowed to perform minor but not major surgery. Any such arrangement as was contemplated in the Bill would be fundamentally affected also by the difference of approach of the osteopath and the medical man. Diagnosis to the medical man meant the ascertainment of what was wrong with the patient; to the osteopath it meant an investigation as to which region of the spinal column was responsible for the origin of the condition.

The examination next turned on the question of the register—not a State register, but a voluntary one, widely recognized by the profession and the public—of the Chartered Society of Massage and Medical Gymnastics.

Lord Elibank: May a masseur on that register treat a patient without going to a doctor?—No.

Why is that? After all, it is not a dangerous operation.—It may be entirely misdirected. Any masseur may practise without being on the register. It is a voluntary undertaking on the part of the members of the society not to treat a patient except under a doctor's direction.

Why not?—Because they do not regard themselves as competent to diagnose cases in which massage is the correct treatment as distinct from those cases in which it is not. But the responsibility of diagnosis having been undertaken by a qualified person, and that qualified person having said that massage is the right treatment, then the masseur is fully competent to apply it.

Sir Henry Brackenbury said that if the American system of licensure of all practitioners of healing obtained here he would withdraw his opposition to the Bill, but the British system was to have one recognized minimum qualification and to set up no prohibition against anybody practising the healing art. State recognition in those circumstances would mean the picking out from all the cults of one or more to which this *cachet* would be given. In this country the public had full liberty of choice.

He was next asked a few questions about the attention given by the medical profession to the prevention of disease. The profession, he said, was always cutting the ground from under its feet. He instanced the virtual disappearance of small-pox and typhoid, the great reduction in the incidence of tuberculosis, the lowering of infant mortality, the elimination of infantile diarrhoea, which was the terror of every summer at the time he began practice—he thought it axiomatic that the medical profession had devoted itself disinterestedly to prevention.

Osteopathy in America

A number of questions were then put to Sir Henry Brackenbury on the position of osteopathy in the United States. He quoted from Publication No. 16 of the Committee on the Costs of Medical Care, a body set up partly by the United States Government—which itself was represented in the inquiry—and partly by the American Medical Association. It appeared that the numbers of practitioners in the United States were as follows:

Osteopaths	7,650
Chiropractors	16,000
Naturopaths and allied groups	2,500
Christian Science and New Thought healers ...	10,000

In the majority of States examinations were conducted and licences granted by independent boards, but in seventeen States osteopaths were licensed by a medical board, in some cases including an osteopathic member and in others without osteopathic representation. In eleven States osteopathic licentiates were definitely denied the right to use drugs; in eight States they were given by statute a limited right to use certain drugs. In four States they were denied the right to use any surgery; in four others they were denied the legal right to practise major surgery; in four, though they were denied the right to use any surgery, specific statutory provisions existed whereby that right might be obtained, and in other States there were various procedures. In fourteen the osteopaths being licensed at present were apparently authorized by statute to practise surgery. Altogether there were a bewildering number of differences in the various States. In twelve the scope of the practice was completely undefined.

He next referred to the Basic Science Laws which had been passed by nine States requiring all practitioners of healing cults to undertake basic science examinations. Save in one of the nine (Minnesota) the examination boards were purely medical. Osteopaths had opposed these enactments, which had been followed in the States concerned by a diminution in the number of cultists. In Nebraska the number of osteopathic licences issued in 1926 was thirteen; the law was passed in 1927, and in 1929 the number of licences was two. In Washington in 1926 the number was thirty-five, and in 1929, after the passing of the Basic Science Law, it was only eight.

The witness included in his evidence an extract from Publication No. 16 of the Committee on the Costs of Medical Care, by Louis S. Reed, Ph.D.:

"In point of numbers, the growth of osteopathy has practically stopped. The failure to grow is probably due for the most part to two things: chiropractic and the evolution of osteopathy. With the coming of chiropractic and the progressive evolution of osteopathy, the osteopaths lost to the chiropractors those patients who were attracted by the 'cure-all' feature."

The same writer went on:

"In general the existence of the osteopathic sect as now constituted presents certain disadvantages. As we have seen, the osteopaths in many States are conducting a general medical practice: in other States, though limited in their scope of practice, they are healing practitioners none the less. Yet as medical or healing practitioners, the osteopaths are not as well qualified as are doctors of medicine. Hence they dilute the quality of medical care available to the people of the country. To-day osteopathy really designates a group of substandard medical practitioners."

Sir Henry Brackenbury affirmed that the growth of osteopathy had practically ceased in the United States. For the last few years the number had been stationary—between 7,000 and 8,000.

Asked with regard to the position in Canada, he said that he had himself visited some twelve Canadian cities, from Quebec to Victoria, and in each had made it his business to inquire, both from medical men and from administrators, as to the present position of osteopathy and chiropractic. It was rather from the sociological than from the medical aspect that he had inquired, as one interested in public health and social welfare developments. Asked whether he could give any reason for the decline in osteopathy across the Atlantic, he said that he believed the best osteopaths had approached so nearly to medical standards that many of those who wished to go in for that form of healing thought it better to take the whole medical course rather than something which cost nearly as much and took nearly as long, and left them with a lower status. With others less scientific or less earnest—he was afraid the majority—the tendency had been to go wholesale into chiropractic, the training for which was cheaper and shorter; there they could make all sorts of wild promises of cure which the better osteopath would not countenance.

Attitude of the B.M.A. to the Bill

Sir W. Jowitt: You do not oppose this Bill because of any effect you think it may have on the profession?—Emphatically not. If we considered the economics of medical practice the Bill, especially in its original form, would probably benefit the profession rather than otherwise.

You do not oppose as such the compilation of a register?—Not at all. It would be useful for those who require to use it.

It is to State recognition and regulation that you take objection?—That is so. And for a number of reasons. State

recognition in this country involves something quite different from a State register in the United States. In this country anybody may practise healing provided he does not pretend to be a registered medical practitioner. Therefore there is no need for a State register to supply a public demand. But if in this country the State picks out one particular cult for recognition it involves consequences quite different from the necessary registration of the various healing schools in America. It gives a certain authority to theories and beliefs which are unaccepted by almost the entire scientific world; it would seem to imply some State guarantee of scientific credit. Even the revenue stamp on patent medicines is regarded by some people as a guarantee that they will do what is claimed for them. Then, again, official State recognition would vitiate the whole purpose of the Medical Acts, which were established to set up a register whereby it would be known that a certain body of persons had received at least a minimum of medical training and to enable the State in its public health activities to use the persons so registered.

Are you to be taken as condemning manipulative methods of treatment?—Not in the least. It is not incompatible with osteopathic methods being successful in a very considerable number of cases, though, I should say, cases of a limited character.

Have those methods of treatment been increasingly recognized of late years by the medical profession?—Yes, and I think the osteopaths deserve credit for underlining them. They have not given us the revelation of a new world, unthought of before, but they have emphasized them. During the last fifty years, speaking broadly of the whole field, I have no doubt there has been too much drugging and too little manipulation, and in so far as the increasing recognition of manipulation and the increasing recognition of limitations in the use of drugs are concerned, I should like to give the osteopaths every credit for emphasizing those points. But they are not new points. The evil of indiscriminate drugging and the value of surgical manipulations were acknowledged and practised in medicine even before the time of Still.

Is it to the extent of the claims the osteopaths are now making that you raise your objection?—Yes, entirely. They are asking not only to have State recognition, but to be in a position to deal with all disease.

The witness went on to say that there was no objection to the application of osteopathic principles in a proper field—though even then there was objection to State recognition of them—but their application to the whole field of medicine was certainly matter for objection. He took various examples of machinery set up by the State for the purposes of public health—the medical treatment of school children, the insurance medical service, medical attendance under the Poor Law, municipal hospitals, and so forth—and pointed out that it would be necessary to provide osteopathic treatment in all these categories.

Lord Esher: There would be no objection to that if the osteopathic theory was true.—If the osteopathic theory be true there would be only one register to be recognized by the State, and that would be the register of osteopaths. The *Medical Register* would be wiped out.

The Two Curriculums

The effect of the Bill (Sir Henry Brackenbury continued) would be to set up an inferior medical education—inferior in several senses. From the date of matriculation or responsions the medical student had now to take a course of not less than six years, and in the immediate future he believed it would be not less than seven. That included the pre-medical work. In the curriculum suggested for the osteopathic school the four or five years would include the preliminary science course and examination, so that it would be a four-years course in osteopathy corresponding to a six-years course in medicine, or a five-years corresponding to a seven-years course. Lord Elibank asked whether it was not a fact that medical authorities were suggesting a shortening of the course, and mentioned Sir Ernest Graham-Little. The witness replied that Sir Ernest had not honoured the General Medical Council with his views on the subject; there had certainly been objection to a lengthening of the course, but there was a strong volume of opinion in favour of the postponement of the beginning of the medical course proper to enable the whole, and not part, of the preliminary sciences to be taken in the pre-medical period. He also objected to osteopathic education because of its compulsory bias.

The Chairman: Is not medical education biased?—I submit that that would be the wrong word to use in that reference. It is unbiased in so far as it takes and teaches all the facts of the basic sciences and anatomy and physiology without any preconceived notions as to certain theories. It is perfectly true that you could say that those who believe the world to be round are biased against those who believe it to be flat, but I do not think you could call a belief that the world was round a biased belief.

Lord Esher: That is only because you yourself believe it to be true!

Sir W. Jowitt put to the witness the possibility that if the Bill were passed the lion and the lamb would lie down together. Of this the witness saw no prospect. The establishment of a register would not lead to closer collaboration in general between osteopaths and registered medical practitioners. It might be all the other way. It would be known that everyone whose name was on that register had had from the beginning a biased education, and held certain fixed ideas, making co-operation rather futile.

Lord Elibank: Have you no fixed ideas in medical science and treatment?—No; except in the sense that if a fact seems to be accepted by the whole or almost the whole scientific world I should not feel that my scientific attainments warranted me in disagreeing with it, but I should hold it the right of a medical man to take any other view.

You say there is no likelihood of co-operation under this Bill, and yet you have told us that osteopaths, or a number of them, are doing good work?—In a limited field.

Can you help me as the promoter of the Bill by telling me how we can bring about co-operation?—There is no bar on one side or the other to consultation with the medically trained osteopath.

Then we can only leave things as they are, and carry on on the chance of something good arising out of the present nebulous conditions?—In essence, yes. There are other means of securing a register, authoritative, but not State recognized, to distinguish between those osteopaths who may be regarded as trained and those who have had no training, and that might enable some medical men to say, "I will seek the co-operation of those on that register."

That is a definite suggestion.—But if the medical man finds abundantly recognized in our own profession men who can do everything for us that any osteopath can do, it seems only natural that we should seek their help.

How many doctors are there who are capable of doing osteopathy and manipulative surgery?—Many hundreds. Certainly more than the osteopaths whose names would be automatically put on the register.

Are they scattered over the country?—That is one advantage. They are scattered all over the kingdom, whereas the osteopaths almost all are concentrated in the wealthier districts.

Cross-examination of Sir Henry Brackenbury

Mr. Murphy, counsel for the applicants for the Bill, began his cross-examination by questioning the authority of the publication of the American Committee on the Costs of Medical Care which had been quoted. He said that there was no indication that the publication, which was the work of one individual, had the imprimatur of the committee. The witness pointed out that, as stated in the report itself, it was submitted in its final form to each of the members, and opportunity was given for dissent, but no dissent was expressed. Mr. Murphy then put it to the witness that the acknowledged rise in the standard of osteopathy in the United States was the result of State action. Sir Henry Brackenbury said that he had no evidence that there had been any State action with regard to the quality of the education of osteopaths, except in the nine States which had passed Basic Science Laws. Counsel also questioned the figures given which showed osteopathy to be a stationary cult in the States, and the witness promised to produce the official figures, which, he said, revealed only a difference of a hundred or two from year to year. The next question related to the possibility of differentiating between certain medical services, and at counsel's request the witness described the procedure under the Insurance Act, whereby every case had to be judged on its merits as to whether it was within or without the contract, and, in the final event of a disagreement between the Panel and Insurance Committees, to be decided by a Ministry of Health tribunal. He was asked whether he saw any difficulty in applying the same sensible practice to the limitations proposed under this Bill, whereby osteopaths would be prevented from doing major surgery. He replied that in his view the differentiation was

quite impossible, apart from the fact that the proposed definition had an American phraseology, the word "physician" being used in the Bill in the American sense; he would substitute "general medical practitioner."

Mr. Murphy: Your conclusion as to the whole matter is that you would leave things as they are at present?—As far as State recognition is concerned.

You have told us that in certain cases within a limited sphere you consider that osteopaths have considerable capacity for usefulness?—Yes.

At present their sphere is unlimited?—Yes.

And you would agree that while they may have capacity for usefulness they may also have considerable capacity for doing harm if they are ignorant or unskilful?—Yes.

And you are satisfied that there are large numbers of people in this country who will continue to resort to the persons calling themselves osteopaths?—Yes.

Are you really satisfied to leave things as they are when the vast majority of the persons to whom the public will resort are unqualified persons?—I am aware of the risk; nevertheless, I prefer the British system to the American.

In the present state of affairs, with the vast preponderance of unqualified persons, the risk is a considerable one to undertake as a sacrifice to British liberty.—I do not think it is a new risk. Every time you indulge in domestic medicine a serious risk is being run.

But you are not comparing that to the case of the unskilled person tampering with the human body?—That is exactly what is being done by anybody practising medicine without scientific education.

That is what a large number of unqualified persons are doing now?—That is what they are doing, and the English law chooses to cover them, and I think the English law will be right.

"You express your views frankly," commented Mr. Murphy.

A Question of Bias

Sir Henry Brackenbury was again taken over the ground already covered concerning the osteopath's fixed idea as a barrier to effective consultation, which led Lord Elibank to interpose:

You stated earlier that you were prepared to co-operate with Dr. Macdonald, but now you suggest that the fact that he has become an osteopath means that he has adopted a fixed idea, which makes co-operation difficult. How do you reconcile the two things?—Perhaps I may be allowed to speak individually. I can conceive of a limited number of cases in which, on my own responsibility, I might ask Dr. Macdonald to do manipulations for me; but the fact that he has adopted certain fixed ideas would weigh with me in considering whether such co-operation would be useful or futile.

You have not the same confidence in him as an osteopath as you would have had before he became an osteopath?—It is not a question of confidence. He is very likely right and I am wrong, but if I believe that the principles on which he acts are not true, that does militate against useful co-operation between him and me.

I understood you to say you would be quite willing to co-operate with Dr. Macdonald, but now you say you would not be willing.—What I meant was that there would be no ethical reason why any medical practitioner should not co-operate to the fullest extent with Dr. Macdonald, but personally my knowledge that Dr. Macdonald has certain fixed beliefs which I do not share would not make me choose Dr. Macdonald as a person with whom to co-operate.

Lord Carnock: And in Dr. Littlejohn's case such co-operation would be definitely wrong?—If it were for the purpose of enabling Dr. Littlejohn to practise as though he were a registered practitioner.

Mr. Murphy: Would your action towards an osteopath be altered one whit if there were a register of osteopaths?—No.

Counsel raised again the question of the "considerable jeopardy" in which an anaesthetist who assisted an osteopath might be placed. Sir H. Brackenbury replied:

You must not ask me to prejudge a case which as a member of the General Medical Council I might be called upon to judge. But it would depend upon the circumstances of each case. I have no doubt whatever that if a registered medical practitioner accepted a salary from an unregistered osteopath to give anaesthetics for him he would run a serious liability of having his name removed from the *Register*.

If a practitioner gives an anaesthetic for an osteopath he knows that his name may come before the General Medical Council?—If he gives an anaesthetic for an osteopath to a patient of that osteopath with whom otherwise he does not have relationship.

If an anaesthetic is to be administered at all it is desirable that it should be administered by a qualified medical practitioner?—Yes.

As things stand at present a registered medical practitioner giving an anaesthetic, which he alone is qualified to do, for the purpose of enabling an osteopath to carry out a useful piece of healing may find himself in considerable jeopardy? Yes; even there I should like to qualify it: his only offence would be if by so doing he was enabling the osteopath to treat the patient as though he were a registered medical practitioner.

Lord Elibank: What do you mean by that?—It is very difficult for me to say, but the offence, if I might call it so, for which the registered medical practitioner might be brought before the General Medical Council is that of assisting a non-registered person to treat patients as though he were registered. If I hold myself at the disposal of the osteopath to step in to do for him in the course of his practice those things which only a registered medical practitioner is allowed to do, then I should certainly be assisting in the carrying on of his practice as though he were a registered medical practitioner.

Lord Carnock: You do not want the registered medical practitioner to act as a stalking horse?—That is exactly the case.

Mr. Murphy: You do not mean that the charge would only be preferred if the anaesthetic were given for the purpose of enabling the osteopath to perform some operation which he was not entitled by law to do?—That would depend upon the facts of every individual case.

Lord Elibank: Suppose he performed an act of manipulative surgery and the anaesthetist helped him?—In my personal view, in an abstract case, I should say that would not constitute a charge on which the General Medical Council would proceed to erase his name. If an osteopath proposes to do a certain act of manipulative surgery, and in an isolated case I give an anaesthetic, I personally, in the abstract, and apart from the circumstances of any individual case, should not hold that that came within the notice of the General Medical Council.

Lord Dawson: The key to the position is that you have sanctioned the diagnosis?—The charge is to be that the registered medical practitioner is assisting an unregistered person to treat patients as though he were registered, and it depends upon the circumstances of every individual case. You may put to me all sorts of abstract cases. All I am anxious to make clear is the nature of the charge which can be brought.

Sir Henry Brackenbury's evidence had not concluded when the Committee rose.

NINTH SITTING

Sir H. Brackenbury's Cross-examination continued

At the sitting of the Select Committee on April 1st, Mr. Murphy, counsel for the applicants for the Bill, continued his cross-examination of Sir Henry Brackenbury.

Taking up the point made by the witness as to the decline in osteopathy in the United States, he referred to a publication of the U.S. Department of the Interior, Office of Education, which gave the number of osteopaths in the census of 1930 as 6,117, and in that of 1932 as 8,169. He elicited from the witness that the diminished numbers he had mentioned in certain States following the passing of the Basic Science Laws related only to new entrants; he had no information that persons already possessed of a diploma in osteopathy had to submit to those laws.

The Theory of Osteopathy

Mr. Murphy recalled that Dr. Kelman Macdonald had put forward certain propositions as underlying the theory of osteopathy, and asked the witness how far he agreed with them.

The first proposition was that structural integrity determines normal functioning. Is there much wrong with that?—Yes, there is much left out. I think that functioning has an effect upon structure probably as great as structure on functioning.

The second proposition was that there is more than one factor in the production of disease?—Yes.

And again, that deranged mechanism can be normalized by manual adjustment or manipulation. Do you quarrel with that?—Not as a general proposition.

Then as a supplement to his propositions Dr. Macdonald referred to keeping clear all nerve paths.—I do not know what that means.

Mr. Murphy then said that if Sir Henry did not know it was of no use a layman like himself trying to explain it. There was some measure of agreement, and he had better leave it there. The witness at the previous hearing, questioned on the subject of bias, had suggested that the view that the earth was round was not a biased view. But the round-earthers in their time had had to suffer a good deal of persecution before their view was accepted. The witness agreed that they had had to prove their case.

If that be the sort of analogy, it would be perfectly unreasonable and useless to require the flat-earther to spend six or seven years in an educational establishment of round-earthers before going to any place of education of his own?—Any man who has a fixed idea cannot get that fixed idea removed unless he has some sort of scientific education.

Re-examination

Mr. H. C. Dickens, in re-examination, asked what sort of bias it was that a medical student got. Sir Henry Brackenbury denied that in the ordinary acceptation of the word there was bias. The students were made acquainted with facts commonly accepted by almost the whole scientific world, and they were taught to keep an open mind.

The next question was the giving of anaesthetics for an osteopath.

If you had a patient who required some manipulative treatment which you thought could best be given by some unqualified person, would there be any difficulty in getting a doctor to give an anaesthetic so long as the responsibility was yours?—Not the least. If I was responsible for a patient and thought that the treatment which he needed was something which an unqualified person should give it would be my duty to ask that person to carry out the treatment.

Therefore a patient who is in need of an operation by an unqualified person could get the operation and the services of an anaesthetist?—Yes.

Lord Elibank: Does that happen in practice?—You have had evidence from the osteopathic witnesses that it does.

Mr. Dickens: There is no ethical reason why an anaesthetic should not be given?—I should not hold it improper, and I do not think any medical authority would hold it improper, for me to give an anaesthetic for an unregistered person if the responsibility for the case remained with me.

The witness was next asked to give the figures on which he had based his statement as to the non-increase of osteopathy in the States. He referred to Publication No. 16 of the Committee on the Costs of Medical Care, which gave the number of osteopaths in 1928 as 7,602; in 1930 as 7,644; and in 1933 as 7,650. An official report of the Commission on Medical Education in 1933 showed the osteopathic schools in America to have numbered thirteen in 1920, which number had been reduced to eight in 1927, and Mr. Dickens said that he could carry it further by a reference to a pamphlet entitled *Osteopathy as a Career*, which showed that in 1934 the number was only six. Sir Henry Brackenbury thought the reduction in the number of schools might be due to the desire of the cultists to recognize only those schools which were the best, so that these figures did not necessarily imply a decline in the number of osteopaths.

Administrative Disadvantages of Two Registers

Sir Henry Brackenbury, in further reply to counsel, made a statement on the administrative difficulties attaching to two registers:

The administrative difficulties of dealing with two State-recognized bodies of persons who exercise the healing art are obvious. Clearly, if the State recognizes two sets of people, wherever the State makes provision for the health of the public it must provide for both those alternative methods. The difficulties of establishing criteria for incapacity for work under the Insurance Act, for example, would be greatly increased if there were two different bodies of persons looking at all these questions from different angles. Again, if, as has been claimed from the chair, osteopaths by manipulating the spine could prevent or greatly minimize the incidence of puerperal sepsis or eclampsia, it would be a wicked thing to deprive prospective mothers of the chance of having their spines manipulated by osteopaths, so that you would have to have at maternity and child welfare centres not merely scientific medicine but the osteopathic healing art, whereby prospective mothers might get their spinal manipulation and so have their predisposing osteopathic lesions eliminated. The same with school children. If it be a fact that the reduction of osteopathic lesions will prevent infectious disease, every small child clearly ought to have its spine examined and its osteopathic lesions rectified before it goes to school. If that be true, and the public desire to have safety in that way, an alternative system of medical inspection and treatmen would be necessary. In every State department there would have to be the same duplication of health activities. Wherever there was a public demand for one kind of State-recognized system as against the other, it seems to me the State could not avoid combining these alternative systems. Local authorities would have to have the option of providing the one or the other even if they did not provide both. Scientific medicine tries to prevent epidemics by dealing with the micro-organism causing the disease; osteopathy would say that manipulation of the spine was the preventive method of choice. These alternative methods would have to be at the disposal of a local authority, and might even become a burning question at municipal elections.

Lord Esher referred to Sir Henry Brackenbury's statement that osteopathy had been repudiated by almost the entire scientific world, but Dr. Macdonald had said that he had made continual efforts to get the scientific world to examine the theory. Was it the habit of the scientific world to repudiate a theory the evidence for which it had not examined? Sir Henry Brackenbury said that he disputed Dr. Macdonald's statement, but he wished to leave that point to the scientific witnesses who would follow. It was inconceivable to him that anatomists and physiologists the world over, having been for a hundred years investigating the structural and functional states of the body, should have failed to observe the osteopathic lesion if it existed.

Lord Esher: If that examination has not taken place you will agree that the opinions expressed are not worth anything?—If the theory which has been brought forward has been accompanied by evidence capable of examination, and such evidence has not been examined, then I agree that those who repudiate it have no ground for their opinion.

Lord Redesdale took up the witness's remark that medical students were taught to keep an open mind, and asked whether that was because the medical profession were aware that they were still groping in the dark. Sir Henry Brackenbury replied: "Yes—in a great many instances."

Lord Redesdale: You are not advancing administrative inconvenience as an argument against recognition of a new method of prevention?—Not if that method can be scientifically proved.

Lord Amulree: Can both methods be true?—No, if the one is true the other is false.

In a severely limited field you agree that the osteopath does good work?—Yes.

In what sphere?—The sphere of the manipulative surgeon, mainly referring to dislocations and structural abnormalities of the joints. I have had patients of my own who desired to go to an osteopath for trouble of the knee-joint or elbow-joint, and they have occasionally been put right.

Does that cover the spine?—There might, of course, be gross structural abnormalities of the joints of the spine in which manipulative surgery would be the right kind of treatment, but in my own experience in general practice the only cases in which I have known my own patients to benefit by going to an osteopath have been a few structural abnormalities of the knee-joint and elbow-joint. I have formed the conclusion that the particular osteopath to whom those patients went was more skilled in manipulating those joints than I. But if my patient, instead of saying that he wanted to go to a particular osteopath of whom he had heard, had asked me for a recommendation I should have had no difficulty in naming half a dozen men within reach, all members of the medical profession, to whom he could go.

Lord Dawson asked whether a useful delimitation of the spheres of osteopath and doctor would be local as distinct from constitutional disease. Sir Henry Brackenbury replied that it would help, but if he limited osteopathic measures to certain forms of local disease it did not remove his objection to State recognition. He added that apparently there were no deaths among patients—many thousands—who had attended the osteopathic clinics in London since 1917, so that in this country osteopaths had no experience whatever in post-mortem examinations, "because nobody ever dies." How they could have a knowledge of disease processes under such conditions he did not know.

Lord Amulree from the chair asked a few questions about the composition of the membership of the British Medical Association, and was informed that it included about 60 per cent. of the practising members of the profession.

At the close of his examination, which had occupied four and a half hours, Sir Henry Brackenbury was thanked by the chairman for the assistance he had rendered the committee.

A Protest from the Chiropractors

Mr. St. J. Raikes, counsel for the British Chiropractors Association, said that as those he represented were opposing the Bill it was not competent for him to cross-examine the witnesses of other opposing bodies, but he did wish to protest against certain statements made by Sir Farquhar Buzzard and Sir Henry Brackenbury. The former had quoted from a book by Professor Haggard entitled *Devils, Drugs, and Doctors*, that "chiropractic is an old form of faith-healing under a new name. Its forerunner was osteopathy." Strong exception was taken by the chiropractors to that statement. Chiropractic had nothing to do with faith cure whatever. The statement made by Sir Henry Brackenbury was that the best osteopaths had approximated so nearly to the medical course that they had, in many cases, gone into medicine, but among the less scientific—Sir Henry was afraid that was the majority—the tendency had been to go over to chiropractic. Mr. Raikes said that he was instructed that a certain number of osteopaths had joined the ranks of the chiropractors, but these were the most intelligent; as regards the others, he would not apply to them the term used by Sir Henry, and describe them as "ignorant," but if the knowledge of osteopathy of some of those opposing the Bill was on a par with their knowledge of chiropractic they would not carry the committee far in arriving at a conclusion!

The Evidence of Dr. J. B. Mennell

The next witness on behalf of the British Medical Association was Dr. James B. Mennell, medical officer of the physiotherapeutic department of St. Thomas's Hospital, and consulting physician to the Red Cross Clinic, Kensington. He said that he started joint manipulation in 1907, soon after he was qualified. He had described the technique and results of his manipulative treatment in several textbooks, one of which was entitled *Backache*. Manipulation was of two kinds—of the soft parts and of the joints. Manipulation of the soft parts came under the heading of massage, though the two kinds of manipulation could not really be separated. There were various ways in which impediment might exist to the freedom of movement of a joint. The movement might be impaired, altered, or fail of completion without any actual displacement. Symptoms arising from that type of condition came under two headings: first, those in the joint itself, from its lack of mobility; and, secondly, referred symptoms. He agreed with counsel (Mr. Dickens) that manipulative methods were confined to affections of the bones, joint, muscles, ligaments, and nerves, though certain effects could be produced on some of the blood vessels. Speaking of asthma, he said that in some forms of that condition manipulative treatment was utterly useless, but there was a type of asthma with a reflex irritation of the nerves from spinal joint trouble which could be relieved and even cured by manipulation. Manipulative methods were of no use at all in acute diseases so far as he knew.

Mr. Dickens: You have been described by Dr. Macdonald as author of a book which reads like an osteopathic textbook. Are you, after all your years of experience of manipulative medicine, an osteopath?—No.

Do you hold the osteopathic theory or any part of it?—No.

You could bring forward as many grateful patients as an osteopath?—I hope so.

Are such cases readily explicable on the grounds on which scientific medicine has been based for many generations?—Entirely.

No mystery?—None whatever.

Have you ever had a success that required the presupposition of anything like the osteopathic theory?—None.

Dr. Mennell explained that his first introduction to manipulative treatment came about through the inspiration of Sir Charles Ballance, who took him over to Paris, and introduced him to the great French surgeon, Lucas-Championnière, who always referred to him as his "English disciple." He came home very much interested in the treatment Lucas-Championnière had instituted—mobilization and massage. On the advice of Sir George Makins he began seriously to consider specializing in physical medicine, of which manipulative treatment was a part. On his first visit to the United States in 1919, at a time when he had never read an osteopathic textbook or seen any osteopathic work, he was invited to address the Association of American Surgeons on the subject of manipulation. He had only seen the work of one qualified osteopath, and he had only seen him treat a few cases. He had read very carefully a book entitled *Osteopathic Mechanics*, which contained no theory, or practically none, and he had read several copies of journals of an osteopathic sort. He thought it a little hard that the work he had done should be described as inspired by osteopathy. If it was alleged that scientific medicine stole from osteopathy, the reverse could certainly be maintained.

An Unfortunate Medical History

Dr. Mennell then described to the committee at some length his own medical history, which had been an unfortunate one, but in none of its various incidents could he trace with any clearness the effect of an osteopathic lesion. In early childhood he had a serious bronchopneumonia. Later, while suffering from scarlet fever, he developed measles and had a dangerous illness. Was the second infection due to an osteopathic lesion of a serious character developing while he was in bed suffering from the first and lighter infection? It seemed to him that the medical theory of weakened resistance was far more probable. He had had mumps three times, a very exceptional occurrence. Either he must have had a spinal lesion which presumably was unique or he must accept the orthodox medical teaching that some individuals were so unfortunate that they failed to build up resistance in their own body to successive invasions by the same micro-organism. In early adolescence he had a series of acute abdominal attacks. Must each of these attacks be accounted for by an osteopathic lesion upsetting the movement of the appendix? In early manhood, following infection in the post-mortem room, he developed a cellulitis of the face and neck. He could not believe that an osteopathic lesion had anything to do with its cause. Later he had a long and disabling illness following acute sacro-iliac strain. If the joint had been manipulated he believed that the illness would have been cut short, but that did not imply an acceptance of the osteopathic creed or theory. Dr. Mennell recounted particulars of other illnesses, and said that if each of them was due to an osteopathic lesion, he must be a "perfect osteopathic freak." It was easier to accept the orthodox explanation. These illnesses included mumps for the third time at the age of 33, general peritonitis, tonsillitis and laryngitis, and tularaemia. At the age of 46 he had a very acute form of septic illness resulting from a "chill." How an osteopathic lesion could allow bacilli to enter the kidney he did not know. He could understand the mechanism from the orthodox standpoint, not from the other. On further examination certain teeth were found to be infected. How it was possible for, say, the first, third, and fifth teeth in the series to be connected with a lesion in a spinal vertebra, and not the second, fourth, and sixth was beyond his understanding. The nerve supply of all the teeth was the same, and started in the skull, not in the neck. Later in life he had occasional attacks of hypoglycaemia, which he cured instantly by eating barley sugar. He anticipated one such attack during his forthcoming cross-examination, emotional strain using up his endocrine reserve—or was he to believe that learned counsel could give him an osteopathic lesion? Mr. Thorpe: "There is no telling what I may do!"

Dr. Mennell said that to assert that no one had investigated treatment by manipulation was rather absurd when he himself had spent thirty years endeavouring to do that very thing. So far as osteopathic theory was concerned, this was a creed, and, as such, incapable of scientific investigation. By instinct and by education he would do nothing to throw doubt upon the creed of another person, which he was bound to respect, and he was a little afraid lest the published report of his evidence should suggest that he was deriding a creed, which was far from his intention.

Dangers in Osteopathic Practice

Even in the limited sphere of manipulation (Dr. Mennell continued) there were dangers unless the practitioner was really skilled. Within the previous week he had seen a spontaneous fracture of one of the thoracic vertebrae as a result

of manipulation in an entirely unsuitable case. For manipulation even restricted to those affections which it was called for, he would accept no training lower than that received in medical schools. The most difficult cases he had to treat in private practice were people who had been led to believe that they had some definite lesion in the spine from which they had not recovered, and presumably never would; the psychological injury was incalculable. Asked what value he attached to a patient's belief in his cure, he said that in organic diseases faith in the cure inspired hope, which was of distinct advantage. In functional diseases everything depended upon the patient. There were many people to whom Beecham's pills were in very truth "worth a guinea a box," simply because they believed in them. It was the faith itself that cured, not the object of the faith, which might be very unworthy of the trust placed in it.

The witness was asked about the click heard during manipulation, and said to be sometimes pointed out by the osteopath as indicating rectification. He said that there were different schools of thought in osteopathy. Some osteopaths would disdain any such suggestion; others would use it, and it was a stamp of ignorance; that was all that could be said. The spinal lesion to the fully trained osteopath was not anything like a vertebra out of place; it was something which interfered with normal function. Personally, he always regarded with suspicion anyone who told a patient that the bone "slipped into place."

Mr. Thorpe: There was nothing about this "click" put to Dr. Macdonald or Mr. Streeter.

Witness: They would have agreed absolutely.

Mr. Thorpe: That is a new way of giving evidence.

Asked whether it was necessary for the osteopathic student to study the whole of the subjects taken by the medical student, the witness said that any medical man who wished to practise a specialty had to be trained in all branches of medicine. His own practice had been confined for years to physical medicine, yet he had had to learn medicine in all its branches, and he was most thankful that he had done so.

Cross-examination of Dr. Mennell

"I do not know whether you are a friend or a foe," said Mr. Thorpe, in opening his cross-examination of Dr. Mennell on behalf of the applicants for the Bill. "You have used the phrase 'a good and properly qualified osteopath.' What do you mean by that?" Dr. Mennell replied, "I think one would accept those who have received admission to the American Osteopathic Association."

And if a British school could bring a student up to as good a level or a better level, you would still rank him as a "good and properly qualified osteopath?"—Yes.

May we assume that there is a collection in this country as well as in America of good and properly qualified osteopaths? What would your professional attitude be? You would co-operate with these gentlemen?—I am afraid I should do the job myself.

Suppose one of them asked you to give an anaesthetic?—I have not given an anaesthetic for twenty years.

Would there be any professional objection to assisting him to that extent?—Not if a medical man sent the patient and desired that the patient should receive——

That is not the question I am putting. Assume that you can give an anaesthetic, and that Mr. Streeter is a properly qualified osteopath according to your standard, would you co-operate with him over a patient?—Not unless that patient had been sent to Mr Streeter by a medical man.

The answer is you would not co-operate. Do doctors send their patients to osteopaths?—They always tell me so.

Do you think the qualified osteopaths fulfil a useful service in this country?—They have helped many of their patients.

I am getting a little nearer to finding out whether you are a friend or a foe. Do you take the view that their theory is founded on a fallacy?—I think so.

If their belief is ill founded their practice is apparently as effective as if it were founded on good sense. How is it they do good? Is it by accident?—May we take some concrete case—asthma? There are undoubtedly cases in which a qualified osteopath would bring help, comfort, relief, and possibly cure, but if I were asked to deal with a case I should have to investigate whether it was a suitable type.

There are certain types of cases in which a qualified osteopath can undoubtedly bring benefit?—Certainly.

And, conversely, cases with which he ought not to be allowed to deal?—Yes.

If you had your properly qualified list you would get over your first fence. If you had your list of things they were qualified to deal with you would get over your second. We will assume a very limited list. That being so, are there any professional reasons why doctors should not co-operate with osteopaths along those lines for the treatment of particular things?—I think we are bound to go back to one guiding law—the law that governs the medical man. The thing which has kept back the whole of this particular branch of physical treatment is the attachment of the practice to the theory. No scientific medical man could accept it, and therefore it condemns the practice.

You follow the practice to a certain extent, but you do not accept the creed?—I practise joint manipulation.

In reply to Lord Elibank, Dr. Mennell said that in such experience as he had had of the qualified osteopath he was able to give the only one he had seen many useful points in manipulation, and he himself learnt certain things from the osteopath.

Mr. Thorpe: There is nothing about the osteopath which keeps him in a world apart?—No, I have sat on a committee with representatives of the medical profession and of osteopaths.

Lord Elibank: What was the object of that conference?—We were discussing with our osteopathic friends the standard of education for the osteopath with which as medical men we would be content.

Had you any authority on that committee?—I was asked to go on; it was quite informal.

Did you arrive at any conclusion?—I do not want to put words into the mouths of others that may not be justifiable.

Yes or no?—Yes, I did.

Did the committee?—No, no definite conclusion—nothing in black-and-white.

Is this continuing?—We are to meet again.

"You have a tremendous amount in common with osteopaths?" said Mr. Thorpe, to which Dr. Mennell replied, "Except their creed."

A distinction has been drawn between local and constitutional trouble. But a local trouble may quite easily lead to a constitutional trouble?—Yes.

You agree that an unrestricted blood flow makes the body healthier than a blood flow which is restricted?—Yes.

What exactly is the fundamental point of difference between the osteopath and yourself?—That I cannot accept the osteopathic spinal lesion as the most important single factor.

Do you agree that it may be a factor in disease?—What do you call disease? Do you call sciatica a disease?

Yes.—Well, of course I should not.

A Confidential Letter

Mr. Thorpe put it to the witness that there was a time he thought something of Dr. Andrew Still. Dr. Mennell agreed that Still was a very remarkable man, though he would not admit that he brought to light the value of manipulative treatment.

Counsel then read passages from a letter written by Dr. Mennell in August, 1931, to a Dr. A. P. Ousdal, a doctor of osteopathy, of Santa Barbara, California. It included the following:

> "In my own mind and in my own heart I pay loyal tribute to the great man [Still] who first conceived the possibility of applying manipulative treatment to the joints of the spine, but you will realize, I hope, that I am wise to try to insist on what one may call the A B C of treatment first, before making any attempt to enter into the wider realms. It was for this reason that I confined my attention in this first edition [of my book] to the one subject of pain."

In a passage referring to orthodox medical people in this country Dr. Mennell had written:

> ". . . you mention one of my illustrations, and add that this is the only place where apparent credit is carried to osteopathy. This is true, and I am sorry to say deliberate, in spite of the fact that I knew quite well of the truth of your statement that every figure and every page [of my book] is a corroboration of the teaching of Dr. Andrew Taylor Still, but you must remember that I was writing in the hopes that I might appeal to the medical men of this country and of the U.S.A. to whom the word osteopathy or osteopathic manipulation is anathema. For them the very word must be dropped, and equally for them the teaching of your great master must be doled out in minute doses and in a disguised form, otherwise prejudice will be so great that again the progress we have already made will be set back for another generation."

Dr. Mennell said that the whole question turned upon the difference between osteopathic manipulation and the osteopathic creed. He admitted that this was not made plain in the letter itself, but it would be understood by the person, a qualified osteopath, to whom he was writing.

You pay tribute to Still, although he was a teacher of heresy?—It is not the manipulation that is the heresy, it is the creed.

May I put it, then, that although he taught a false creed, you pay loyal tribute to him?—Still was a very remarkable man and a great teacher, deluded, as I think, by his faith.

You say in the letter " . . . before making any attempt to enter into the wider realms." What are the wider realms?—The wider realms of detail.

You write: " Some day I hope that it will be possible for more to follow, but the time is not yet " (you are still writing of osteopathy). That would not lead your correspondent to expect that you might write a book condemning osteopathy?—I never have.

In order that he might not be accused of selecting passages Mr. Thorpe then read the whole letter. It contained the following:

> " Of course you will know in your own country how long and how bitter has been, and indeed still is, the fight of orthodox medicine against all manipulative work. The fight has been a thousand times more strenuous in this country, and any attempt to preach osteopathy as such is doomed at the outset to condemnation. . . .
>
> " The attitude that I have always adopted is that orthodox medicine in which I was trained has a useful and invaluable purpose to serve, but that the less orthodox methods have no less their own sphere of usefulness, and that until the two are combined they will neither of them rise to perfection, and that therefore neither of them will serve their fullest purposes."

The concluding paragraph of the letter read:

> " I hope that you will consider the views that I have expressed here as confidential; as, were they broadcast, I am afraid that my first effort to accustom my professional brethren to the idea that manipulative treatment has its useful place would be seriously handicapped. So far the great point gained is that I have been able to produce an osteopathic textbook, however rudimentary, which has received appreciative notice in every medical journal which, so far, has published a review. My general attitude may be jesuitical, but I hope it is none the less justifiable. At least I am grateful to you for your letter of appreciation and encouragement. Yours sincerely, (signed) James Mennell."

Dr. Mennell made a strong protest against a letter of a confidential character being divulged. He had expressed to a private correspondent certain views in a less considered manner than he would have done before the present tribunal.

Mr. Thorpe: You say in the letter that you have gained a great point in that you have been able to produce an osteopathic textbook. That was meant to bring joy to the osteopath's heart that someone was blazing a lonely trail in this unhappy country.

Dr. Mennell: It is my honest and true belief that when the osteopath is combined with the medical practitioner we shall have a very much more valuable armament than in either of them separately.

Re-examination

Sir William Jowitt confined his re-examination to a few leading questions. Dr. Mennell agreed that in a large number of complaints manipulation was very necessary. In the past at any rate the medical profession had been slow to recognize the necessity. In part this hesitation was because the method of treatment was bound up with a particular theory, and so far as the theory was concerned he had never accepted it. If the theory was right, manipulative methods would be the correct method of treatment for all diseases. " So the lonely trail which you blazed many years ago and of which my friend has spoken," said Sir William Jowitt, " is now a well-lit thoroughfare."

Dr. Mennell: My profession has been accused of being slow in accepting treatment by manipulation. The glory of our profession is not so much the cure as the prevention. No medical man is justified in prescribing a cure unless he has good, solid scientific reason for prescribing it, and while treatment by manipulation was attached to an incredible creed I do not believe members of my profession were justified in prescribing it.

Lord Esher asked whether the witness accepted the medical theory of immunity seeing that he had had mumps three times, the theory being that one could not have mumps more than once. He replied that there were exceptions to any theory. Lord Marley asked some questions on the preventive value of manipulations. Lord Dawson said that the value of manipulative treatment for local conditions was not seriously in question, whether it was called osteopathy or not. The main issue concerned the osteopathic claim that all diseases fell within its ambit. Lord Elibank asked whether the manipulative treatment which Dr. Mennell gave was merely a craft or founded on science. Dr. Mennell replied that it was entirely founded on science and observation. Lord Elibank then argued that if osteopathy was based on scientific thought and observation it was entitled to some recognition in the science of healing. Dr. Mennell replied: " Not by attachment to a creed which is not susceptible to scientific proof or disproof." In reply to further questions, Dr. Mennell said that some of his osteopathic colleagues on the committee he had mentioned were definitely in favour of the full medical curriculum, with manipulations as a post-graduate study.

Before he left the chair Dr. Mennell said again that he was disturbed by the letter which had been read. He had forgotten the occasion, but the letter was undoubtedly written in entire confidence to a man who presumably had written to him in very friendly terms about his book and to whom he had replied with similar friendliness. Lord Elibank said that he did not think the letter went any further than Dr. Mennell had done in his principal evidence.

Lord Dawson said that he had received a telegram from Dr. Macdonald promising that the scientific evidence for osteopathy, consisting of the seven Bulletins of the A. T. Still Research Institute with some additional material collected from journals, should be in his hands by April 5th.

The Committee adjourned until April 5th.

CARBON-DIOXIDE-OXYGEN TREATMENT OF ASPHYXIATION

The Mines Department has issued a circular (No. 76) to owners, agents, and managers of mines under the Coal Mines Act, the secretaries of national and local associations of owners, officials and workmen, and the managing bodies and superintendents of central rescue stations, concerning the use of carbon dioxide gas in cases of asphyxiation, including carbon monoxide poisoning. This follows the issue on October 24th, 1933, of a circular (No. 64) in which the Secretary for Mines drew attention to the advantages of this method of treatment.

It is stated that steps will be taken in the near future to amend the now out-of-date requirements of the Rescue Regulations, which deal with the provision of reviving apparatus: in the meantime the Secretary for Mines urges owners to make the necessary provision for apparatus without waiting for the formal amendment to the regulations. CO_2 can be delivered from a separate cylinder, or a mixture of CO_2 and O_2 in the proportion of 7 per cent. to 93 per cent. can be administered from one and the same cylinder. It is easily possible to adapt the ordinary reviving apparatus, delivering only oxygen, for the administration of the two gases, provided that a larger breathing bag and tube is fitted, together with a suitable mask, and that a device is fitted to control the mixture and indicate its composition.

The instructions are that a person found unconscious in an atmosphere vitiated by carbon monoxide should be removed to a safe place in fresh air, but that he should have treatment before being brought out of the mine. He should be covered with blankets, and, if necessary, placed in a prone position, and Schafer's artificial respiration started at once. The reviving apparatus should then be applied, and administration continued without interruption until spontaneous breathing is established, or at least for fifteen minutes. If CO poisoning is suspected, the supply of the mixture should be continued for a short time—five to ten minutes—after natural breathing is restored. The patient should be kept under observation until arrival of the doctor.

REHABILITATION OF THE DISABLED

AN AMERICAN EXPERIMENT

A large number of people interested in the surgical and economic aspects of industrial accidents were invited to the Western Electric Theatre, Bush House, on March 28th, for the projection of a sound film, the work of Dr. H. H. Kessler of Newark, New Jersey, depicting the work of the Rehabilitation Commission in that State. The film—an excellent example of cinematograph publicity—illustrated the manifold methods and generally successful results of this great State enterprise, the chairman of which is Professor Fred H. Albee, late president of the American Orthopaedic Association.

The progress of the victim of an industrial accident was traced, from the moment when first aid was applied to his final rehabilitation in some remunerative employment. In the rehabilitation building at Newark every equipment for orthopaedic reconstruction and vocational training of the handicapped is installed, and in an atmosphere which breathes encouragement many of the crippled show a surprising adaptability to new conditions. In the most recent year 289 physically handicapped persons were placed in remunerative work by the commission. In addition, it has made a survey of all cripples in New Jersey up to 18 years of age, and by co-ordination with existing agencies, the enlistment of the medical profession, and the provision of orthopaedic service and vocational guidance, many are being enabled to look forward to a self-supporting career. The economic as well as the humanitarian value of the work was stressed.

EXPERIENCE IN BRITAIN

Following the demonstration a brief discussion took place. Mr. E. S. Evans, chairman of the Organization Committee of the Cripples' Training College at Leatherhead Court, Surrey, said that it was hoped to have in that college something on the New Jersey model to serve London and the Home Counties. The scheme for the college was launched in March of last year at a meeting at the Mansion House, and the work was already going ahead as far as available funds permitted, but it was to be officially opened by the Duchess of York at the end of June, by which time it was hoped that the remainder of the necessary funds would have been raised.

On the general question, Mr. Evans criticized the interpretation of the Workmen's Compensation Act in this country, whereby treatment and rehabilitation were lost sight of in money payment. One New Jersey experience was specially valuable: employers often hesitated to engage a man who had once had an accident, because it was feared that he was accident-prone, but the work of the Rehabilitation Commission revealed that accident-proneness among those who had been disabled was considerably less than among their able-bodied colleagues. Mr. Geoffrey Peto, M.P., suggested that the Government should appoint a commission to hear evidence on this subject of rehabilitation, and to make recommendations as to the respective functions which Government departments, employers, and trade unions might discharge. In this matter, he said, we lagged behind not only America but European countries, like Austria and Belgium. Mr. McAdam Eccles also expressed the view that the time had come for another interdepartmental committee.

Mr. R. C. Elmslie said boldly that the question of workmen's compensation ought to be taken out of the hands of lawyers, and he was supported in this by speakers from the trade union point of view, one of whom said that the whole service of workmen's compensation ought to be administered, as it was in New Jersey, by a public body. The same speaker said that the average cost of running workmen's compensation in this country represented 37½ per cent. of the premiums paid, whereas with a State scheme as in New Jersey it represented only 5 per cent., with a ten times better service. Answering some oblique criticism of insurance companies in this connexion, a representative of one of them said that the difficulty was that while certain insurance corporations might very well be inclined to put up considerable sums for rehabilitation work they were held back by the consideration that their action would be benefiting other companies which were not contributing at all. A comprehensive policy was called for whereby failing a friendly arrangement which would bring in all parties, the compulsion of the law should be sought.

England and Wales

Cost of L.C.C. Medical Services

The estimates of expenditure for the financial year 1935–6 recently submitted to the London County Council included £417,715 on capital account for general and special hospitals, and £5,263,735 on maintenance account for these and other public health services. This is £268,795 more than the provision made in 1934–5, and £720,746 more than the actual expenditure in 1933–4. Many improvements are proposed, especially an increase in hospital staff and installation of better equipment, also improved arrangements for dealing with tuberculosis, dental affections, and the domiciliary treatment of public assistance patients. Included in the estimate is £58,435 for pathological laboratories, £38,550 for the district medical service, £115,900 for the diagnosis and treatment of venereal diseases, and £31,750 for maternity and child welfare. The Mental Hospitals Committee submitted estimates for expenditure on capital account of £221,890, and on maintenance account of £2,693,705—an increase of £53,000 over last year. The estimates were approved by the Council.

Purity of Milk Supply in London

The London County Council has been considering, through its various committees interested, the conclusions and recommendations of the Economic Advisory Council's Committee on cattle diseases, of which Sir F. Gowland Hopkins was chairman. The Hospitals and Medical Services Committee is in agreement with the recommendations regarding power to prohibit the sale of "milk (uncertified)"—that is, ordinary milk which has not been heat-treated—after some date not earlier than five years from the initiation of the scheme of eradication of bovine tuberculosis. It also endorses the requirement as to the official approval of pasteurizing plant, and strongly supports the recommendation for the standardization of tuberculin. The Mental Hospitals Committee also supports the proposal for the standardization of tuberculin, but urges that pasteurization should be regarded not as a substitute for a clean milk supply, but only as a necessary precaution under existing conditions. Other recommendations, such as those which call for facilities to be given to farmers for testing milk samples for the chronic streptococcal form of mastitis, and emphasizing the need for research into Johne's disease and *B. abortus* infection, have the support of the Council.

Conference on Prevention of Tuberculosis

Further details have been received regarding the twenty-first conference of the National Association for the Prevention of Tuberculosis, to be held at Southport from June 27th to 29th, under the presidency of Sir Robert Philip. A discussion on the safeguarding of the child against human infection in the home and school will be opened on the morning of the first day by Dr. G. Lissant Cox and Dr. Skokowska-Rudolf of Warsaw, associate secretary-general of the International Union against Tuberculosis. This will be followed by a discussion on the safeguarding of the child against bovine infection, with reference especially to milk supply, introduced by Dr. R. Veitch Clark, Professor Christiansen of Copenhagen, Major A. B. Mattinson, F.R.C.V.S., and Dr. Nathan Raw. In the afternoon of June 27th there will be a debate on the question "Can the child be immunized against tuberculous infection?" the first two speakers being Dr. Edouard Rist of Paris and Dr. K. Neville Irvine. The morning of June 28th will be devoted to consideration of the ways in which the child may be

safeguarded against the risks consequent on tuberculous infection. Dr. Charles Cameron of East Fortune will deal with the steps to be taken to determine the fact of infection at the earliest possible moment, while Drs. L. S. T. Burrell and H. Midgley Turner will urge the systematic examination of children from time to time. In the afternoon the significance of sanatorium (preventorium) methods and open-air schools will be emphasized by Dr. Ralph Williams, and Drs. C. J. Thomas and Noel Bardswell. After these discussions the conference will merge into the annual meeting of the National Association, which will include the exhibition of new cinema films produced by this organization. On Saturday, June 29th, visits will be organized to various tuberculosis institutions of the Lancashire County Council. Special social and recreational facilities will be accorded to members of the conference by the Corporation of Southport, including an evening reception and dance.

Ireland

Royal Victoria Hospital, Belfast

The report of the Board of Management of the Royal Victoria Hospital showed that admissions for the year totalled 6,512, the highest figure yet recorded. Of this total approximately 4,240 patients, or 65 per cent., were admitted as emergency cases, 830 being the result of accidents, including 204 motor accidents. The number of cases awaiting admission at the ending of the year was 1,202, being 177 more than on December 31st, 1933. It is hoped that as the result of recommendations approved by the Board of Management the waiting list will not be further increased. The medical report of the hospital stated that the total number of deaths during the year was 472 and of these ninety-eight had occurred within forty-eight hours of admission. In the ophthalmic ward 2,806 cases had been dealt with. The financial statement showed that the balance at debit of revenue account on January 1st, 1934, was £232 19s. 6d. The disbursements for the year were: Royal Victoria Hospital £57,908 and Throne Hospital £3,147 4s. 10d. ; the receipts were: Royal Victoria Hospital £59,109 4s. 4d., and Throne Hospital £683 18s. 6d. The outstanding feature on the receipt side of the account was the increase of £3,684 2s. 5d. in the workpeople's subscriptions, which amounted to £25,122 11s. 5d. This was very encouraging, reflecting as it did the improvement in unemployment, which, it was hoped, would continue. Two projects referred to in the report were: (1) a pension scheme for nurses ; and (2) the provision of a pavilion for any patients under the Musgrave Bequest. The Royal Maternity Hospital had accommodation for forty pay-patients at the present time. That accommodation was used for nurses' quarters, and until the latter could be housed elsewhere the hospital could not be utilized to its fullest extent. The Board was anxious to proceed with the latter scheme, so that a grant under the Unemployment Act amounting to approximately £12,000 could be secured.

School Inspection in Fermanagh

A comparison of the figures for defects in the recent annual report on the medical inspection of school children in County Fermanagh by Dr. W. A. Dickson shows that the tonsil and adenoids and eye treatment schemes have to a great extent dealt with the vast accumulation of cases which were on hand, and the stage has now been reached when school medical inspectors have chiefly to deal with fresh cases as they arise. The same happy position does not yet exist with regard to dental treatment ; although there are now three part-time dental surgeons there was such an accumulation of cases that it will be still some time before much can be done in the way of conservative treatment. Except in the matter of infectious diseases, Dr. Dickson states, the general improvement in each and every department of school life is very gratifying, and, above all, there has been a great improvement in the matter of school accommodation in some areas. The co-operation of the parents in the matter of their children's health can be readily understood from the fact that since the scheme started the total number of refusals for medical inspection has not yet reached double figures. Among the statistics which Dr. Dickson gives are the following: Total number of children examined, routine and special, 2,756 ; special dental inspections, 3,043 ; defective teeth, 1,683 cases ; defective vision, 382 ; tonsils and adenoids, slight, 433, marked, 355 ; cervical glands, 282 ; and otitis media, 145.

Northern Ireland: Vital Statistics

The Registrar-General reports that of the 5,935 births registered during the last quarter of last year 3,086 were of boys and 2,849 of girls, the total being equivalent to a rate of 18.5 per 1,000 of the estimated population. This rate is 0.1 below the rate for the corresponding quarter of 1933, and 0.4 below the average rate for the fourth quarters of the ten years 1924–33. Of the 4,301 deaths 397, or 9.2 per cent., were of infants under 1 year. This number is equivalent to 67 in every 1,000 births registered, as against 71 for the corresponding quarter of the previous year, and an average rate of 72 for the corresponding quarters of the five preceding years.

Scotland

Tuberculosis in Scotland

At the annual meeting of the Royal Victoria Hospital Tuberculosis Trust, which was held on March 28th, Sir Robert Philip, who presided, said that this Trust aimed at co-operating with the health authorities as fully as possible, but dealt with cases that would hardly ever gain admittance to the ordinary institution. When the cause of tuberculosis was discovered in 1822, he continued, the mortality from the disease in Scotland stood at 311 per 100,000 of the population ; last year it had fallen to 75. In many European countries, such as France, Spain, Portugal, and Austria, the figures of mortality were still several hundred per 100,000 of the population, while in Denmark, on the other hand, the figure was down to 54, and in Belgium and Holland to almost the same number. These were countries which had made a vigorous onslaught on tuberculosis. The report of the Trust mentions that £5,000 has been allocated for the purpose of investigations, and the excellent sources of the laboratory at Southfield Sanatorium are being utilized in full degree. The tubercle-free herd maintained by the Trust now numbers 110 cows, and demonstrations are regularly given to medical, veterinary, and agricultural students.

The report of the East Fortune Sanatorium for the year 1934 shows that the admissions, numbering 214, were thirty-five fewer than in 1933. The number of discharges was 188, and the number of deaths thirty. Of the patients admitted 51.4 per cent. were suffering from pulmonary tuberculosis, 41.1 per cent. from other forms of the disease, and 7.5 per cent. were found not to be suffering from tuberculosis. Of 107 patients discharged

from the institution after treatment for tuberculosis of the lungs 20 per cent. had the disease in a state of quiescence, 48 per cent. were improved, 8.5 per cent. were in a stationary condition, 2.8 per cent. were worse, and 20 per cent. died. An after-care scheme is conducted, under which patients are seen on two occasions during the year.

Heredity in Mental Deficiency

At the annual meeting in Glasgow of the Royal Scottish National Institution (for mental defectives), Larbert, Dr. R. D. Clarkson, medical superintendent of the institution, said the most important event of the past year had been the establishment of the adult colony for mental defectives. When he first became associated with this work only a small number of people were interested in mental deficiency, and they viewed the matter chiefly from the religious and philanthropic aspect. Now, however, the public was not so much interested in mental defectives as in mental deficiency, and was taking a very lively interest in this problem. The numbers in the Larbert Institution had increased, but he did not believe that mental deficiency was on the increase ; so far as he could note there did not appear to be more mental defectives to-day than there were when he started the work over forty years ago. Mental deficiency was largely a matter of definition, for it was a social inadequacy due to low intelligence which resulted from some arrest in the development of the brain. It had been proved that mental deficiency of such severity as to be recognizable in the first two or three years of life had very little relation to heredity, and, in his opinion, there was still complete ignorance of the extent to which mental deficiency was attributable to heredity. The financial statement of the institution showed that there was still a capital balance of £12,000 in hand, of which, however, a large portion would be required for extensions.

Glasgow Samaritan Hospital

Presiding at the annual meeting of the Glasgow Royal Samaritan Hospital for Women, the Lord Provost, Sir A. B. Swan, stated that the ordinary income of this hospital was always about £3,000 short of its expenditure ; this had again been the case in 1934. During the year a new wing had been opened, and preparations were being made for a paying-patient department. Professor J. M. Munro Kerr said that during the year great advances had been made in the equipment of the hospital ; a radiological department had been built and was now functioning, while a whole-time pathologist had been appointed to take charge of the research department. He appealed to younger members of the medical profession to think not only of ordinary hospital duties, but to give their spare time to furthering research and to making clinical observations that would advance the study of gynaecology and obstetrics.

Memorial to Dr. Haig Ferguson

At the annual meeting, on March 29th, of the Lauriston Home, Edinburgh, which is conducted in connexion with the Maternity Hospital, it was decided to change the name of the institution to the Haig Ferguson Memorial Home, and the Dowager Countess of Moray unveiled a memorial plaque in the entrance hall. This bears a bronze relief portrait of Dr. Haig Ferguson, who was one of the founders of the home, and who for over thirty years acted with great enthusiasm for its support and as physician to its inmates. A number of friends since his death subscribed a sum amounting to over £3,000, which will be used to form an endowment towards the upkeep of the home.

Reports of Societies

FUTURE OF RADIOLOGICAL DIAGNOSIS

At the meeting of the Medical Society of London on March 25th, with Lord HORDER in the chair, the subject of discussion was the future of radiological diagnosis.

Dr. H. GRAHAM HODGSON said that in his recent presidential address to the Royal Society Sir F. Gowland Hopkins had referred to the altered conditions under which the general practitioner of to-day did his work. Formerly the practitioner depended principally on his eyes, ears, and hands, but the application of physics and chemistry to the study of disease had placed at his disposal technical procedures of the utmost delicacy. The discovery of *x* rays was one great step towards precision in diagnosis. But no man could possibly be a clinician, a biochemist, a bacteriologist, and a radiologist all in one. There must be division of labour here as elsewhere. People often complained when they went to a general practitioner that they were drafted on to a specialist ; but specialism in medicine was inevitable.

The possibilities of future progress in radiology depended almost entirely upon the men practising the specialty. The essential pre-requisite of the radiologist, as A. E. Barclay had written, was sound knowledge of anatomy and clinical medicine. Essentially the radiologist must be a clinician, and the wider his knowledge of clinical medicine the more would it be to his advantage as a radiologist. The old tradition whereby *x*-ray departments were lodged in basements and in charge of porters and nurses still left a partial shadow over this specialty, and radiologists to-day suffered from that early lack of foresight. Although radiology had won its place as a specialized and indispensable branch of medicine, there still existed hospitals where the radiologist was not admitted as a full member of the honorary staff, and the specialty occupied the position of a crown colony rather than that of a dominion in the empire of medicine. Every new discovery and every new claim in the profession had to pass through a long period of trial, criticism, detraction, and even abuse, before it was accepted—rightly so in a profession founded on a scientific basis. Perhaps that was why the medical profession was not popular with the Press, politicians, or the osteopath. Popularity, however, was only another name for what was sensational, new, and untried. But radiology as a special branch of medicine had been proved, tried, and found worthy. It was now at the parting of the ways. Was it to go on progressing on an uphill path or be content to maintain its present level? The answer depended upon the radiologist himself. If it was to make progress the coming radiologists must equip themselves with a wider and more extensive basis in pathology and clinical medicine. A few years ago the anatomical scope of *x* rays was much more limited than now. To-day the radiologist had constantly to deal with difficult and widely different problems. No ancillary branch of medicine was as widely used. It was intimately connected with every specialty of the healing art, with the possible exception of psychiatry. The wider the radiologist's clinical knowledge the greater the assistance he would render his colleagues. He must be able to talk to the neurologist, the laryngologist, the bacteriologist, and all the other members of a highly specialized profession in their own language, so that not only might he assist them as far as possible within the present sphere of radiology, but, having a full knowledge of their difficulties, might be constantly endeavouring to devise means of application so as to embrace a solution of their difficulties. Radiology was the study of contrast and density. Almost daily *x*-ray apparatus was improving, so that more anatomical detail could be visualized. The radiologist of the future, if he was to do his subject justice, must be as highly trained and qualified as any of his colleagues. The speaker predicted that in the not distant future, hospitals, in advertising for a radiologist, would expect the candidate to have the same higher qualifications as were expected of the physician,

surgeon, and pathologist. Turning to apparatus, Dr. Graham Hodgson said that it had improved out of all recognition as compared with that in use only five years ago. The vast majority of the improvements had initially come from abroad, not because British engineers were less capable than foreign, but because *x*-ray manufacturers in this country were divided into five or six small firms which competed amongst themselves instead of combining to oppose successfully the great combines of America and Germany. The future progress of *x*-ray technique, he believed, would take place along two main lines: increased anatomical detail due to the application of teleroentgenography and an increasing use and refinement of opaque materials. In this connexion he described a new tube called the Siemens Pantix rotating target tube, which had six focal spots, each of the smallest possible size. Each focal spot, although very small, bore only a fraction of the brunt of the total discharge, so that a very much heavier current could be passed through this tube without injury. With this tube there went the advantage of the finest possible focus, and yet a heavy current could be sent through it, so that it could be used advantageously for teleroentgenography, where heavy currents were needed. The Middlesex Hospital *x*-ray department would shortly be equipped with one of these tubes. He had hoped to have some films to show that evening, illustrating the value of this rotating tube, but he could only show some radiograms taken with an old tube having a particularly broad focal spot, and he called attention to the great improvement in definition with distance up to six or seven feet. The radiograms gave some idea of what would be possible when this new introduction got to work. The cost of the tube—£200—was rather forbidding.

The use of radiopaques was extending over a very wide field. Great progress had been achieved by the introduction of intravenous pyelography. Recently some very useful work had been done in the study of the mucosa of the gastro-intestinal tract by small quantities of barium coupled with a technique whereby the rugae were outlined. Another useful method was the study of the colon by filling with barium, which was afterwards siphoned off, and air injected, so that abnormalities, such as polyps, which the barium itself had not revealed, became visible. Was the use of barium the last word in the examination of the gastro-intestinal tract? It was on a different plane from cholecystographic examination, in which advantage was taken of physiological function. A possible future line of progress might be to ally the use of opaque media with physiological action. Absorbable opaque substances might be taken by the mouth, possibly mixed with fats, proteins, or carbohydrates, and their rate and area of absorption studied. More use might be made of the laws of gravity in radiological study, and the erect posture more often utilized. One day television would be applied to *x* rays. The most obvious advantage would be to allow two men to study the same *x*-ray presentation without either of them leaving his separate consulting room. More usefully by this means it would be possible to brighten to a quite marked degree the screen image without increasing considerably the screening current. In *x*-ray cinematography much had been achieved, though much remained yet to be done. He referred with appreciation in this connexion to the work of Dr. Russell Reynolds, and showed one of Dr. Reynolds's films illustrating movements of the heart and lungs.

Dr. RUSSELL REYNOLDS briefly described his technique (*Journal*, February 23rd, page 381). He mentioned that he had now covered 300 subjects belonging to three categories—namely, orthopaedic, the thorax, and the gastro-intestinal tract. The number of new facts ascertained by the cinematographic method was quite extraordinary. Beyond the initial cost of the apparatus, the method was not expensive. A great deal could be shown on fifty feet of 16 mm. film, the cost of which was 6s. 6d. Recently he had been able to correlate electrocardiographic tracings on film showing the heart action.

Dr. FANE TIERNEY said that it was often urged that the radiologist must be a clinician, but it was sometimes ignored that he must be also a technician. Technically *x*-ray work was often badly done, not due to any fault in the apparatus, but to the neglect by the radiologist of the technical side as against the diagnostic. So far as small lesions were concerned in the more radiologically accessible parts of the body, *x*-ray pictures taken in the early years of the present century compared favourably with those taken to-day. In teaching radiologists the emphasis was laid upon academic physics, which was of little use when it came to attending to a motor or a generator, and not enough attention was paid to technical radiology, with the result that the radiologist could get his diploma although very defective in technical acquirement. Dr. A. C. SHANKS suggested the joints as a useful future field for contrast medium. There should be some contrast medium which could be injected intravenously and secreted into a joint. Something of the kind had been done, but not very successfully. He regarded cineradiography as one of the greatest advances in *x*-ray diagnosis that had ever taken place. At the moment it was to some extent experimental, but was rapidly coming into the field of practical diagnosis. Slow-motion cineradiography of the heart, in which the cardiac movements might be studied at leisure, might be very useful. Dr. PETER KERLEY defended radiological training. The course for the Cambridge diploma was for nine months, with three months on technique. Students were required to do a certain amount of technical work on their apparatus. They got thirty lectures on technique, and, furthermore, they must spend three months at least in the radiographic department of a teaching hospital and submit a thesis with suitable pictures. He thought technical education was reasonably well covered. He went on to draw attention to fallacies in *x*-ray cinematography as applied to diseases of the heart. It seemed a beautiful thing to watch the heart contracting on the film, and it gave an impression of actuality. But a cinematographic record was a collection of single pictures pushed through very rapidly, and until those pictures had been synchronized with the action of the heart it was a false representation of movement. Only if the actual taking of the radiographs was carried out by the impulse of the pulse at the wrist or elsewhere was it a true record.

Dr. RUSSELL REYNOLDS, in answer to this, said that if the cinematographic record were taken rapidly enough one was bound to get the whole movement—the record was almost a continuous one. Records were being made now at the rate of thirty pictures per second—that is, with exposures of one-sixtieth of a second. Dr. KERLEY, however, still maintained that unless there was absolute synchronization with the pulse itself there was liability to grave error. More of Dr. Reynolds's films were projected in order that members might judge for themselves.

Lord HORDER said that he had been struck by Dr. Graham Hodgson's insistence on keeping as far as possible to physiological action in the use of opaque media. He fully agreed that the closer the radiologist stuck to physiology the more helpful would be the interpretation of his results.

HYDATIDIFORM MOLE

At a meeting of the Section of Obstetrics and Gynaecology of the Royal Society of Medicine on March 15th, with the president, Mr. EARDLEY HOLLAND, in the chair, Mr. ALAN BREWS gave a follow-up survey of the cases of hydatidiform mole and chorion carcinoma treated at the London Hospital since 1912—seventy-two of the former and sixteen of the latter.

Twenty-seven of the patients with mole were over 40 years of age—that is, 37.5 per cent. In one case there was a twin pregnancy, a foetus attached to a normal placenta being expelled, and, in addition, a hydatidiform mole, which seemed to be entirely separate. The majority of the cases (fifty-nine) came complaining of uterine haemorrhage, but some came because of vomiting, abdominal pain, swelling of the feet, headaches, or abdominal enlargement. Usually the duration of amenorrhoea was one to three months, but in 10 per cent. there was no history of previous amenorrhoea. In the thirty-seven cases in which the duration of the pregnancy and the size of the uterus before any mole was

expelled were recorded, the uterus was unduly enlarged in twenty-four, smaller than normal in eight, and the correct size in five. Of the thirty-four cases in which the urine was examined albumin was present in 35 per cent. Treatment was usually by manual expression, digital separation, and removal with ovum forceps. In eight cases abortion was induced by laminaria tents, and in one case by a rubber bag. Vaginal hysterotomy was done in one case, subtotal hysterectomy in four, and panhysterectomy in two. In the seventy-two cases there was only one death directly attributable to the molar pregnancy. The patient had three months' amenorrhoea and daily bleeding for eight weeks. She was moribund on admission, and died before a transfusion could be given or the mole evacuated. Six cases out of the seventy-two developed chorion epithelioma, an incidence of 8.3 per cent. In the last two cases of the series the Friedman and Zondek-Aschheim tests were employed.

Professor JAMES YOUNG, in relation to the question whether a true malignant condition was possible in hydatidiform mole, referred to a case described by James Miller some years ago in which after death the lungs were found to contain chorionic villi, showing marked invasive properties. Mr. BLAIKLEY did not accept a perforating mole as a case of chorionic epithelioma, and did not consider perforation of the uterus to be evidence of malignancy.

Birth of a Chimpanzee

Mr. JAMES WYATT gave an account of the birth of a chimpanzee at the Zoological Gardens, London.

The ape was 12 years old, menstruation began at the age of 8, and occurred regularly every twenty-six to twenty-eight days, lasting about four days. The last period was from June 4th to 8th, 1934. Coitus took place each day during the ten days from June 11th to June 20th. Towards the end of August she began to get fits of rage, in which she screamed, threw herself about, and bit her arms and legs. Calcium sodium lactate, 7 grains, t.i.d., seemed to control these. At the end of December, when the breasts began to enlarge, she pulled on the nipples every day, evidently with the idea of drawing them out. Labour set in about 2.30 p.m. on February 15th, 1935, and during the "pains" the ape passed her fingers into the rectum or vagina, probably with the idea of moving the cause of the trouble. Mr. Wyatt first saw her at 6.15 p.m., when the pains were regular every five minutes. At 7.15 the foetal scalp began to show during a pain, and the mother tried to hurry matters by pulling on the scalp. With three or four more pains the head was crowned, and now the ape's fingers were passed right round the head, and she rapidly pulled it out, and then grasped the neck and extracted the body. The placenta was retained until about thirty-six hours later, but eventually came away spontaneously, though the ape had apparently been much worried by the umbilical cord hanging out of the vagina, and kept making attempts to pull it away, which she eventually did with the help of a piece of cotton-wool. Except for a temperature of 99.5° F. for the first two days the puerperium was uneventful. The baby made no attempt to suckle, so thirty-six hours after birth it was put to the breast, and since then had fed every three or four hours. After an interval of about ten minutes the mother changed the baby from one breast to the other.

THE USE OF ANTISEPTICS

At the meeting of the Section of Odontology of the Royal Society of Medicine on March 25th the subject for discussion was "Some Factors in the Use of Antiseptics."

Professor A. FLEMING said that antiseptics might be defined as chemicals which would prevent the growth of bacteria; they might kill the bacteria, but that was not at all necessary. Practically all chemicals were antiseptics if used in sufficient concentration, but in practice the term was limited to such chemicals as had been shown to have, or claimed to have, a special potency in this direction. Common salt was not generally claimed as an antiseptic, but it was used possibly as much as any other chemical for its strictly antiseptic properties, and salt herrings and salt beef proclaimed its merits. In medicine antiseptics seemed to have a sacred name, which was well kept up by the manufacturers in their advertisements. In the history of surgical procedure antiseptics had a setback during the "aseptic" period before the war, but in the early part of the war practically all the surgical authorities abandoned their aseptic principles and followed antiseptic methods, though no great advance was made in antiseptic production. At the beginning of the war carbolic acid, iodine, and perchloride of mercury were the antiseptics that were in the minds of those who had to treat septic wounds. The onset of sepsis encouraged chemists and others to produce new antiseptics, and much work was done which might have taken a more useful direction. In the war Dakin's fluid, flavine, and bipp were used in particular. The laboratory with which he himself was connected during the war (the research laboratory at No. 13 General Hospital at Boulogne) tried to elucidate the septic wound and to show that it was impossible that any chemical could do what was asked of it. Their preachings fell for the most part on deaf ears, except that Sir Almroth Wright's advocacy of hypertonic saline had a considerable following. This was not a war teaching, but had been proclaimed long before the war began. A later modification was the magnesium sulphate paste and glycerin, now used in suppurative conditions of the uterus and ear.

Antiseptics (Professor Fleming continued) could be used both outside and inside the body, and different standards and tests were required to ascertain their value in each of these fields. Outside the body antiseptics were used for a great variety of purposes, many of which had no connexion with medicine at all—for example, sterilization of soil. The sterilization of excreta was an important subject from the standpoint of public health. The sterilization of instruments was another important application. Instruments could easily be sterilized by antiseptics provided measures were taken to ensure that the antiseptic reached every part of the instrument. This question of efficient contact was of very great importance. Strong antiseptics could be used outside the body, and the chief problem—not an easy one—was to ensure that the antiseptic obtained access to the bacteria. Inside the body the concentration of the antiseptic which it was possible to employ was strictly limited. The action of the tissues and the body fluids in hindering the antiseptic action had to be taken into account, also specificity, rapidity of action, affinity for tissues, and penetrative power. Probably there was much more general use of antiseptics in the mouth than anywhere else in or on the human body (though, of course, the mouth cavity was really outside the body). A large variety of tooth pastes and powders were available, most of which, according to the advertisements, were possessed of great antiseptic powers. Mouth washes, too, were advocated because of their power to kill bacteria. Probably most odontologists would agree with him with regard to tooth pastes and mouth washes that the antiseptic action was a minor one, and that the benefit was derived from the cleansing action whereby bacteria and other particles were mechanically washed away. Professor Fleming showed the results of certain experiments as to the possibility of sterilizing a cavity with antiseptics. He said that the indirect actions of chemical "antiseptics" might be important, as well as the effect of antiseptics in general on the natural antibacterial mechanism of the body. The present tests in common use for the assessment of the relative potency of antiseptics gave no indication of their value inside the body, and new tests should be adopted. Healthy human blood was nearly constant in its composition, and furnished a suitable medium for a test of antiseptics in the human body. This medium gave not only a measure of the antiseptic power of the chemical, but also an indication of its toxicity to human blood cells, which were the principal antibacterial agents in the circulating blood

Dr. L. P. GARROD asked those present to consider whether they were prepared to be guided by the results of laboratory tests at all. These were commonly ignored or derided, and antiseptics were judged in practice only by the clinical effects with which they were credited. Iodoform and mercurochrome were two antiseptics which had been shown by tests to be quite inefficient, but they continued to be used freely. Iodoform had no action whatever on pyogenic cocci. Streptococci grew on the surface of a mass of bipp in broth. Walker had shown that staphylococci would grow in blood containing 1 in 400 mercurochrome. If tests were to be accepted as a guide, a standard form of appropriate tests for a surgical

antiseptic was needed. The slide-cell method advocated by Professor Fleming gave results which indicated that every known antiseptic did more harm than good, and it was a test applicable to conditions in the circulating blood or within the tissues. Prophylaxis by means of antiseptics was a different matter from the treatment of established infection, and as a guide to efficiency for this purpose a test should be satisfactory which used blood only as a hindrance to disinfectant action, and without regard to the behaviour of its leucocytes. There was ample evidence from animal experiments that streptococci could be killed in the tissues, infection being prevented, if flavine or similar antiseptics were introduced within an hour or two of the introduction of the streptococci.

In subsequent discussion Mr. J. G. TURNER asked whether Professor Fleming had ever tried the specificity of N.A.B. against the *viridans* organism, either in the mouth or intravenously. At the Royal Dental Hospital it had been found useless, but others said it was a success. He added that in actual dental practice at the hospital iodoform had for a long time been given up. The president of the Section (Mr. FRANK COLEMAN) said that bipp was used as an antiseptic during the war, but with doubtful results, and later its use was countermanded. He thought that Sir Almroth Wright had hardly received his full share of credit for his introduction of hypertonic saline solutions for the treatment of wounds, a method which assisted the natural processes of combating bacterial infection. Professor FLEMING, in reply, said that he had once tried to find out what virtues bipp possessed, and his conclusion was that it was not an antiseptic at all; yet it seemed sometimes to do good. He had been pleased to hear some praise of Dakin's solution. During the war it was found the most successful plan to instil it every two hours. He had made some investigations to discover how long Dakin's remained in the wound, and these showed that five minutes after being put in it ceased to be an antiseptic.

CHOLESTEROL IN HEALTH AND DISEASE

At the meeting of the Section of Medicine of the Royal Society of Medicine on March 26th, with Sir FARQUHAR BUZZARD in the chair, the role of cholesterol in health and disease was the subject of discussion.

Dr. HUGH GAINSBOROUGH said that as a result of recent work a new molecular constitution had been formulated for cholesterol, and its previously known chemical relation with the bile acids had been extended. Similar relations had been demonstrated with the male hormone, oestrin, ergosterol, and calciferol. Thus the sterols played many very different parts in the metabolism, though there was so far no clue as to the occurrence of any intermediary chemical transformations between these substances in the body. Cholesterol existed in the body in the so-called "free" form, and also combined with fatty acids as esters. Both forms were present in plasma and most tissues, but the "free" form alone was present in brain, erythrocytes, and the bile. The functions of cholesterol in the cell economy were little known, but that it could be synthesized by the organism was now generally accepted. Sterol was excreted in the faeces as coprosterol and β-cholesterol mainly, except in infants, where cholesterol alone was found. Herbivora could absorb cholesterol avidly, and did not excrete it, and their tissues then became infiltrated with lipoid material. Cats and dogs excreted less coprosterol and more cholesterol than man. Coprosterol was formed in the large intestine supposedly by bacterial action on cholesterol; the significance of its formation was not known. The cholesterol of the plasma showed normally some degree of variation, and in health values outside these limits were found only in pregnancy. In pregnancy certain definite changes occurred. Up to the thirtieth week there was a progressive increase of "free" cholesterol and a decrease of ester cholesterol, after which the values returned to normal level by the first week or two of the puerperium. Hypercholesterolaemia occurred in nephritis when associated with marked oedema, in complete biliary obstruction, in some forms of xanthoma, and sometimes in diabetes; but in a large number of cases of subacute nephritis, where the average of plasma cholesterol must have ruled high for prolonged periods, there was no tendency to gall-stone formation. The normal secretion of cholesterol in the urine was 1.7 to 4 mg. per day. In hypothyroidism the total cholesterol in the plasma might be increased up to 50 per cent. above normal value; after administration of thyroid the proportion became normal. Hypercholesterolaemia was said to occur in fever, but the differences from the normal were small. Changes of the cholesterol content of the plasma were as yet unexplained, and there was no evidence that hypercholesterolaemia was a retention phenomenon.

Dr. R. D. LAWRENCE discussed the effect of hypercholesterolaemia in diabetes on the production of arteriosclerosis. It was accepted that diabetics, especially those of long standing, were particularly liable to arteriosclerosis, and tended to die from that rather than from any other complication. The view that hypercholesterolaemia in diabetes caused arteriosclerosis probably started long ago in Aschoff's laboratory, but it had been largely taken up in the big American clinics. The manifestation of this arterial degeneration was most commonly seen in gangrene and thrombosis, in retinitis, and in a secondary albuminuria. It was quite true that in severe diabetes the cholesterol in the blood was increased very often to three or four times the normal limits, and ran strictly parallel with the severity of the case. These cases also showed other disturbances in fat metabolism, and were always accompanied by ketosis and a general elevation of the total lipoids in the blood. He had never himself been very much impressed with the relation of this excess of cholesterol to arterial degeneration, and had always been inclined to think that if the other factors had been as easily estimated as cholesterol, fatty acids or lecithin might equally well have been blamed in diabetes for the arterial changes. As regards prognosis, a high cholesterol in an untreated case meant nothing; it was amenable to regulation. There were, to his mind, several reasons for thinking the cholesterol hypothesis in this connexion unproven. The changes found in these vessels in diabetes were confined to the intima; it was the medium-sized and peripheral vessels that suffered, and the most obvious change was calcification. It was also noteworthy that these arterial changes occurred in the long-standing mild cases; it was the elderly diabetic who had never had a very severe diabetes in whom gangrene, retinitis, and so forth occurred, and in such cases there was no altered character in the fat metabolism and certainly not a raised blood cholesterol. In his view, after an observation of cases of diabetes of all degrees of severity, the emphasis placed on cholesterol and abnormal hypercholesterolaemia in diabetes as a cause of arterial degeneration was not justified.

Dr. C. E. NEWMAN said that the importance of this subject lay in the fact that cholesterol was excreted by the liver and was an important constituent of the bile and prominent in most gall-stones. The relation between gall-stone formation and cholesterol metabolism was very close. Two principal types concerned were the faceted stone, together with the barrel stone, two or three of which were often articulated, and the pure cholesterol solitaire; pigment stones had no cholesterol at all. The first type and its variants comprised the overwhelming majority of gall-stones which caused clinical symptoms. The mechanism of their formation was still very largely unexplained. The two factors were the cholesterol origin, the substance being in an insoluble form, and the tendency to form a stone, not simply a mass of crystals which could easily be washed away. Unfortunately, the experimental animal most commonly taken was the rabbit, whose cholesterol metabolism was entirely different from that of the human being. The only animal which produced gall-stones at all like human gall-stones was the pig, and if the work were repeated on the pig some useful results might be forthcoming. Cholesterol was certainly absorbed from the intestinal canal, and an increase of absorption led for a time to a raised blood cholesterol; but afterwards the body readjusted itself, because the amount of cholesterol obtained from food was relatively insignificant compared with the extreme importance of cholesterol to the organism. To his mind the internal synthesis of cholesterol was very much more important than the small

amount of cholesterol to be found in food. About one-third of the cases of high blood cholesterol had a high bile cholesterol, and one piece of information still wanted was the relation between a high blood and bile cholesterol and the occurrence of a cholesterol solitaire. It had been recommended that people with gall-stones should be cut off cholesterol; yet many cases of gall-stones benefited by the judicious administration of fats. He did not think it right to cut people off fats—for that was what it amounted to—if they had gall-stones. He saw no reason why such patients should not be given olive oil. This acted as a nourisher, and was useful in emptying the gall-bladder. The emptying of the gall-bladder by fatty meals was an important part of the treatment of any condition in which the bile was being altered in its composition. A pregnant woman should not be cut off cholesterol if she had a longing for it.

Dr. F. PARKES WEBER took exception to Dr. Gainsborough's remark that there was no evidence that hypercholesterolaemia was a retention phenomenon. He recalled the cases, less often seen now than formerly, of chronic obstructive jaundice, with sheets of xanthomatous skin over the neck and other parts of the body. The liver secreted cholesterol. What more natural and to be expected than that, when the ducts were blocked up and there was retention of the biliary secretion, hypercholesterolaemia, among other things, should occur? Its permanence was a different matter, but it occurred, and lasted long enough to produce a deposition of cholesterol to an extreme degree in many cases, in the tissues.

Sir JAMES DUNDAS-GRANT asked whether it was desirable to withhold fats in order to restrain the development of senile deafness. Some French workers had suggested the importance of avoidance of cholesterol-holding food, such as brain, liver, yolk of egg, and fats. Was there justification for cutting off eggs and bacon in subjects with a tendency to senile deafness?

Dr. GAINSBOROUGH, in reply, said that there had been too much tendency to draw conclusions from supposed minute changes in the cholesterol in the blood. He thought that dieting would make very little difference in senile deafness, and in gall-stone patients there was no reason whatever in the present state of knowledge to withdraw fats from the diet. Many cases tolerated fat and improved under a high fat intake. The hypothesis under which pregnant women were deprived of cholesterol was very ill supported.

CHOCOLATE CYST OF THE OVARY

At the March meeting of the Section of Pathology of the Royal Academy of Medicine in Ireland, with the president, Dr. W. R. O'FARRELL, in the chair, Professor J. T. WIGHAM showed a specimen of double chocolate cyst of the ovary.

The patient gave a history of having had some supposed appendix trouble for eighteen months. This had been gradually getting worse, and on admission she said she thought the time had come when she would have to have an operation. The abdomen was opened and a perfectly normal appendix found. The ovaries were both enlarged, and contained a large quantity of chocolate-coloured fluid. One ovary had a thick wall, but the other had a thin wall.

These tumours, Professor Wigham said, were supposed to arise from portions of endometrium, which, with a certain amount of blood, had come out from the Fallopian tubes. The ovary did not look like a place in which cells could easily be implanted and in which they would grow quickly and successfully. The cells in this case were mostly round cells until the spindle-celled stroma of the ovary was reached.

The PRESIDENT said that these cysts were very interesting, because there were so many of them and so many explanations of how they occurred. He thought the idea that they were endometriomas was rather far-fetched. Although there were so many cysts of the ovary it was very rare to come across endometrial cysts with an actual lining which was endometrium. These follicular cysts often had a low columnar lining. If a specimen was received labelled "chocolate cyst of the ovary" was it to be described as an endometrioma, or was "chocolate cyst without malignancy" a sufficient description?

Dr. F. S. BOURKE said he had seen a few chocolate cysts in which endometrial tissue was present, but this had only occurred in about three cases out of the many he had examined. He would be very slow to describe a specimen as a chocolate cyst unless there was endometrial tissue in the thicker portion of the wall. Dr. J. MACGRATH said he had seen some cysts which might be described as chocolate, and in any specimen he had examined he had failed to find evidence of tissue which might be regarded as being strictly similar to that of the uterine mucosa. In a number of cases he found the blood separated in layers. This was not seen in any of the cases in which blood pigment was found throughout the wall, and it seemed to him that the fluid was being absorbed into the wall of the cyst.

Professor T. T. O'FARRELL, referring to the origin of chocolate cysts, said he had frequently looked for endometrial tissue in the ovary with negative results. The ovary appeared to him to be an organ which would be more likely to repel that to attract cell grafts. He commented upon the appendicular symptoms in this case, and drew attention to the fact that the appendix and right ovary had some common blood supply, so that ovarian trouble might readily be referred to the appendix.

Professor WIGHAM, in reply, said he thought chocolate cysts did exist, and were cysts of the ovary which contained dark-coloured blood. The fact stated by some observers that they were endometriomas had not been definitely established, and yet it was impossible to say that they were not. He thought that a good many chocolate cysts were endometriomas, while others certainly were not. His impression was that the blood in these cysts had been some time collecting, and was not recent; it was not the blood which was found in a luteal cyst in the ordinary way.

"AMNIOGRAPHY"

At a meeting of the North of England Obstetrical and Gynaecological Society held in the University, Liverpool, on March 22nd, with Mr. A. GOUGH, the president, in the chair, Dr. F. J. BURKE read a paper entitled "Amniography."

In recent years, he said, it had been found possible to diagnose placenta praevia by means of the *x* rays. The importance of this discovery was not widely appreciated, in spite of the need for improvements in the method of diagnosis. The literature was briefly summarized, and then Dr. Burke described some of the difficulties, and gave the results of twenty-three cases. There was placenta praevia in five of these, as shown by *x* rays. There did not appear to be any gross risk to the mother. Of the five neo-natal deaths and stillbirths in his series four could be accounted for by natural causes. An injection of uroselectan into the uterus tended to induce labour. Dr. Burke described twenty-seven cases in which uroselectan was used for this purpose. Five abortions were also induced by means of uroselectan. He referred to the types of cases in which uroselectan injection was useful, and described the ultimate fate of the injected medium. He found that a great amount was excreted by the kidneys.

Dr. C. H. WALSH said that injection of uroselectan into the uterus had the following uses: (1) to show up the foetus in early pregnancy; (2) to demonstrate the position of the placenta; and (3) to induce abortion or labour. He emphasized the fact that the method was entirely without risk to the mother and child.

Mr. J. ST. GEORGE WILSON said that he had used the method in a few cases and had not been entirely successful, but this was because the *x*-ray films were too small. Professor MILES PHILLIPS thought that the method described might be of value in some cases. Professor D. DOUGAL asked if it was possible to show the position of the placenta by a simple *x*-ray examination without the previous injection of any opaque substance.

Control of Urinary Incontinence

Professor LEYLAND ROBINSON and Mr. M. DATNOW described a case of vesico-vaginal fistula of unknown origin, with a new apparatus for the control of urinary

incontinence. The patient was aged 45, a two-para, and had had her last child fifteen years previously. She was two years after the menopause, and came to the hospital complaining of urinary incontinence of only one month's duration. Examination revealed a large vesico-vaginal fistula about half-way up the anterior vaginal wall. No growth was present. Urinary investigation for tubercle bacilli proved negative, and the Wassermann reaction also was negative. Uroselectan injection showed complete absence of function of the right kidney, with hydronephrosis on the left side. The left ureter was dilated in its whole length. Operation failed to close the fistula. The speakers demonstrated the apparatus which the patient had found very satisfactory for controlling her urinary incontinence. They had also inserted the instrument in other cases with satisfactory results. It consisted in the main of a ring pessary with a diaphragm, to which was vulcanized a rubber catheter, whose end was inserted into a bag that was worn on the patient's leg. The instrument had to be removed and cleansed each week.

Avulsion of Uterus

Miss G. GRIFFITH described a case of complete avulsion of the uterus during an attempt at manual removal of the placenta. The patient was delivered by forceps of a stillborn infant. No difficulty was encountered. The practitioner attempted to express the placenta, and then decided to remove it manually because of the excessive bleeding. On removing her hand from the vagina, she found to her amazement that it was the uterus which she held in her grasp. After the patient's admission to hospital and treatment for shock, laparotomy was performed and the peritoneum sutured. Miss Griffith demonstrated the specimen.

Professor DOUGAL described a case which was reported to the Obstetrical Society, and said that very little force was needed to tear away the uterus. Professor PHILLIPS had collected thirty-six cases of avulsion of the uterus, fifteen of which had recovered. It required little or even no force to remove the uterus. In these cases there could be no doubt that more or less complete separation of the uterus from the vagina spontaneously occurred. In other cases there was evidence of varying degrees of violence.

Hydatidiform Mole

Mr. T. N. A. JEFFCOATE described a case of hydatidiform mole with negative Zondek-Aschheim reaction. He drew attention to the fact that undue enlargement of the uterus was not an invariable accompaniment of hydatidiform mole, and his case was an example of this. The Zondek-Aschheim test carried out with the urine in this patient gave a negative result. Mr. Jeffcoate thought that this was due to the fact that the mole was dead and the chorionic elements inactive.

The PRESIDENT described a case of typical hydatidiform mole in which the Zondek-Aschheim test had been negative for some unexplained reason. Professor GOUGH suggested the possibility that the urine in some cases of hydatidiform mole contained so much hormone that the test animals did not show any reaction unless the urine was diluted first. Professor PHILLIPS emphasized the fact that it had long ago been brought to the notice of the North of England Obstetrical and Gynaecological Society by Professor Briggs that in many cases of hydatidiform mole the uterus was smaller than expected. Dr. GERRARD said he had recently seen an example of mental disturbance being the result of hydatidiform mole.

The second general meeting of the Midland Mental Pathological Society was held at Birmingham University on March 28th, when thirty-four members were present. Papers by Dr. D. L. WOODHOUSE, on "The Chemical Composition of the Brain," and by Dr. J. H. QUASTEL, on "Brain Enzymes," were well received. Membership of the society is open to the medical and surgical staffs, pathologists, bacteriologists, and biochemists attached to the general or mental hospitals of the Midlands. Particulars can be obtained from the honorary secretary, Dr. F. A. Pickworth, Hollymoor, Northfield, Birmingham.

CORRESPONDENCE

History of Vitamins

SIR,—In his interesting study of human nutrition in your number of March 23rd Sir Frederick Gowland Hopkins, doubtless inadvertently, appears to convey an impression that the origin of our knowledge of the vitamins is of more recent date than many of those who are engaged with medical work in the Tropics have been taught and have long been accustomed to teach. Under the heading "The Vitamins" he says:

"Some twenty years ago, however, the realization came that the economy of the animal body calls for the activities of substances with functions apparently akin, in many respects at least, to those of the hormones, which the body itself is nevertheless unable to produce, and therefore must receive them in its food. The indispensable functions of these, like those of the hormones, are adequately fulfilled by extraordinarily small amounts of each one. These food constituents yield, therefore, no appreciable supply of energy, nor do they serve in any ordinary sense as structural materials. Their presence, like that of the hormones, is necessary rather for the normal progress of active events. They have dynamic functions. I am alluding, of course, to the vitamins."

The expression "twenty years ago" does less than justice to the most important work of Eijkman on avian polyneuritis and beri-beri, which appeared as early as 1897, and to the subsequent work of Stanton, Fraser, and many others who contributed to our early knowledge.

Again:

"I must refrain from following up that gradual accumulation of evidence which led first of all to the discovery that substances of the kind are necessary, and later to a realization of their multiplicity and to our growing knowledge of their functions. I think I may assume that you are not unfamiliar with the general nature of that evidence. It began with discriminative feeding experiments on animals, and the results of these became soon linked up with evidence from the study of so-called deficiency disease displayed in human communities."

The order of events given here was hardly the order in which the events are recorded; apparently Eijkman first observed, fortuitously, that certain fowls were affected by polyneuritis; he then traced the cause to their diet of prison polished rice, and from this point he began his experiments on fowls and his experimental observations on human beings; he showed that the symptoms were due to a deficiency in the diet brought about by the loss of vitamin B due to removing the germ and pericarp and subpericarpial layer.—I am, etc.,

D. B. BLACKLOCK.

Liverpool School of Tropical Medicine, March 27th.

Gastrectomy

SIR,—So gastro-enterostomy is dead; "and none so poor to do him reverence," though the obituary admittedly is rescinded in the case of chronic obstruction with gastric atrophy and of irremovable pyloric carcinoma. Furthermore, the operation ("disease," says the cynical physician) had had its excesses but little less renowned than those of the appendix, the tubes, ovaries, uterus, tonsils, and nasal septum. Given even these facts, have we the full story?

Certain statistics allege an astoundingly high ulcer morbidity, and others merely refer to discomfort without imputing ulceration (a distinction with a difference, for a large number of these have not an ulcer but peri-anastomotic cellulitis, as evidenced, for instance, by

stippling); while others clearly show (for example, the B.M.A. and British Association of Surgeons) that a very high proportion of anastomoses "get away" with a comfortable life for an indefinite period. If the procedure is *so* unphysiological and dangerous it should be invariably unphysiological and dangerous (as most certainly it is in bad cases of anastomotic ulcer). Surely the decisive factor is appropriate selection of cases, concerning which emphatically the last word has not been said.

Why is it that a simple anastomosis performed merely as a temporizing measure for a huge infiltrating prepyloric ulcer results frequently in complete, apparently unending, relief, although the test curve shows anything but achlorhydria? Should one in such cases perform a *prophylactic* gastrectomy? Surely a low mortality non-mutilating procedure should, wherever possible, take precedence of a comparatively high mortality mutilation. It cannot be doubted but that partial gastrectomy is followed by permanent relief in a much greater proportion of cases, but the correspondence, with one brief exception ("8 per cent. in the hands of the greatest Continental exponents"), has been ominously discreet about the cost. Throwing everything into the opposite balance—the imperfect reaction of gastric ulcers to anastomosis, to resection, and to other local treatment; the still not-quite-negligible cancer-ulcer question; and, very importantly, the usual lack of appropriate after-treatment—one must have the very strongest grounds for adopting, instead of a 2 per cent. mortality operation, one which, I venture to assert, *in general* has at least a 15 per cent. mortality.

With regard to the question of anaesthesia (one has used all methods) one thing is certain: in general the British patient and his surgeon prefer him to "know nothing about it." It is unnecessary to point out the factors in shock production: the presence of "shrieking" tissues after the anaesthetic has passed off, whatever it be, and however carefully administered, will still cause shock and produce the reflex pneumonic conditions so to be feared in upper abdominal operations. The essential is non-tractive, non-bruising surgery with the minimum of repetitive handlings and fumblings. But it must be pointed out that sitting up in bed next day and asking for the paper is no prerogative of gastrectomies under local anaesthesia, and the same may be seen any time with a straightforward resection under well-administered ether, the total time necessary on the table being at the outside from forty-five to fifty minutes. Two hours is another matter, and then undoubtedly some form of nerve block becomes imperative.

For the technical side of Mr. Ogilvie's article one can have nothing but admiration. I should like to point out that I have for six years practised and demonstrated prepyloric resection for ulcer, having devised it for precisely the reasons Mr. Ogilvie names—greater suitability of stomach wall for suture, avoidance of smaller vessels, stump on which one is working being nearer to the surface, etc.—though I have never succeeded in making the suture look like the pretty dimple seen in the illustration.

In conclusion, it still seems necessary to reiterate that the treatment of gastric and duodenal ulcers (certainly those occurring in surgeons) is medical. From the large number of operative cases reported it would seem either that our medical colleagues are singularly inefficient or that few of the patients possess the steely resolve necessary to attain the gastronomic Nirvana essential in the regimen. Or can it be that the basal pathology is still utterly misunderstood, that our concentration on the sore in the stomach wall and on the acid curve is leading us off that trail, and that when understanding really comes the question of gastrectomy *versus* gastro-enterostomy will be swept away by the abolition of both?—I am, etc.,

London, W.1, March 25th. C. JENNINGS MARSHALL.

The Omental Plug in Stomach Perforation

SIR,—I have read with interest the contribution of Mr. C. Leonard Isaac (*Journal*, February 9th, p. 252). I have never used the exactly identical technique which the writer describes—that is, that of threading the omental plug on a suture and passing this through the perforation and through a healthy portion of the stomach wall; nor have I ever seen it used in England or on the Continent. The idea of passing this anchoring suture through the stomach wall does not appeal to me. Mr. Isaac must be familiar with the omental graft for closure of large perforations surrounded by much induration and congestion where simple inversion of the ulcer is not practicable. The method is described in all the commoner textbooks of surgery, not to mention those especially devoted to operative surgery.

I have had two cases within the last twelve months where I found the perforation so surrounded by friable stomach wall that invagination of the ulcer was impossible. The method which I have practised for some time now is to select a portion of the omentum having a good blood supply. A row of interlocking sutures is then placed on either side of the selected portion, and two incisions made on either side internal to the sutures, so that a free limb of omentum is raised complete with its blood supply. This is turned up on to the surface of the stomach; the undersurface of the graft is then sutured by interrupted sutures to the healthy stomach wall around the indurated area. Next, the upper surface of the omentum is also made fast to the stomach slightly outside the previous circle of stitches. The cut edges of the graft are then attended to with a few stitches to close the edges. The procedure is long of description but facile of execution, and is, I think, surgically sound.

I have not yet had the opportunity of reopening an abdomen in which such a procedure has previously been carried out. It would be interesting to know what happens to this graft, and whether there is any possibility of its causing obstruction.—I am, etc.,

A. T. ANDREASEN,
Captain I.M.S.

Indian Military Hospital, Jhelum, Punjab, March 11th.

Jung's Influence on Clinical Psychiatry

SIR,—In your observations under the above heading in the *Journal* of March 23rd (p. 596) you ask: "Is it, therefore, to be assumed that Jung has really made no contribution to clinical psychiatry *in this country*?" (italics mine). Jung's ideas and method of approach have had a far-reaching effect throughout the German-speaking world, and British psychiatrists can scarcely hold him responsible for the fact that they remain unaware of his writings.

With regard to the meeting of the Psychiatric Section to which you refer, if Dr. Mackenzie's paper had been announced as "Variations on a Theme Suggested by Jung's System of Types" it would, I think, have represented the substance of his interesting paper, and the whole question of Jung's contributions to psychiatry would not have arisen. As it was, the expectations kindled by his too ambitious title were left unsatisfied. As to my own part in the discussion, I could not hope to fill the gap that yawned between Dr. Mackenzie's title and his paper. To present, in a brief impromptu, a bird's-eye view of the immense terrain covered by one of the most creative minds of to-day during a lifetime of systematic exploration is an impossible feat, and no one regrets more than I do the lame inadequacy of the attempt.

The question you raise does, however, demand a more explicit response. Thirty years ago Jung was working as an institutional psychiatrist in the Burghölzli Asylum in Zurich. His work on word-associations and his psychological study of dementia praecox were done under the aegis of a great institution. The whole trend of his investigations led him to the view that there was an archaic unconscious background of the mind from which the psychotic disturbance arose and towards which psychiatric exploration should be directed. His collaboration with Freud marked the end of his first phase as institutional psychiatrist and introduced him to the wider field of clinical psychological research. Jung's break with Freud was again forced upon him by the uncompromising nature of his discoveries ; for the impersonal or archetypal contents of the deep unconscious could not be compressed into the too-narrow conception of the Freudian unconscious. In his study of dementia praecox Jung emphasized the toxic factor, but this did not delude him into thinking that toxicity spelt dementia praecox. He realized that there was a general archaic mental background, the contents of which are normally wholly unconscious, but which, under certain mental and physical conditions, can profoundly disturb, or even take possession of, the conscious field. Thus he was led to his formulation of the collective unconscious, and, arising out of this, to his generalized concept of the libido. The concept of libido as psychic energy went necessarily hand in hand with the idea of the opposites, and Jung's conception of psychic equilibrium as being the expression of the reciprocal action of a fundamental affective duality paved the way for a genuine psychological approach to the whole psychiatric field.

Having penetrated deeply into the uniform historical archaic levels of the psyche, Jung now approached the human being once again from the angle of the differentiated psychic functions and general psychological types, extending his description of the function types and the complementary mechanisms of introversion and extraversion from clinical studies to the operation of the same principles over the whole sphere of culture. I believe the key to a fuller appreciation of Jung's work in this country lies in the acceptance of an unprejudiced psychological empiricism. An empirical investigator in any field of science has to deal with its specific phenomena by means of descriptive concepts derived from the material native to that field. Jung's writings are full of clinical studies, which range from the fullest description of morbid processes to parallels between the therapeutic process observed in his patients and long-established psychological systems in the East.

Even from this sparse catalogue of Jung's contributions —and only a fraction of them have been included—it would seem incredible that English psychiatry should still affect to ignore his significance. But it is all the more amazing when the almost unredeemed sterility of orthodox English psychiatry is borne in mind. Whether it is that the prevailing extraverted bias of English mentality produces a resistance which renders Jung's introverted psychological approach unassimilable, or whether the spirit of scientific rationalism that inspired, for example, Dr. Mapother's presidential address finds in Jung's arational basis of the mind an altogether insuperable obstacle, it is indeed hard to say. But the fact remains that Freud and Jung together have given to psychiatry a psychological, in place of the old physiological, foundation ; and there can be no shadow of doubt, the " thunders of Denmark Hill " notwithstanding, that the psychiatry of the future will achieve its fullest potentialities in dealing with the problems of mental suffering on the basis which these great pioneers have laid down.—I am, etc.,

London, N.W.1, March 25th. H. G. BAYNES.

The Riddle of the Psychoses

SIR,—the object of Dr. Suttie's original letter on " The Riddle of the Psychoses " was to discuss the question of research (March 9th, p. 505). Some of the more recent writers on this subject have tended rather to discuss treatment. I should like to refer back to the subject of research.

Dr. Suttie suggested that mental hospitals, under their present system, were unsuitable as centres of research, and gave as his reasons: (1) That these hospitals isolate the patient from (*a*) his own environment, the possible cause of illness ; (*b*) the doctor who is investigating the riddle. (2) That psychotics are unwilling to associate themselves with an " asylum," hence we fail to attract those who, by nature of their illness, are most accessible to investigation and treatment. Whether or not the environment is the primary cause of the psychoses is open to debate. Also, I do not see that the isolation of the patient interferes with research. Reason (1) (*b*) cannot be denied. The medical officers are so overburdened by administrative and regulation duties that if they have time to devote to research they are fortunate. Reason (2), remembering we are talking of research, is not a sound one ; from the point of view of treatment it is a different matter. All mental hospitals have psychotics ; therefore the material is there awaiting the investigators. It is doubtful whether more information will be obtained from these cases in their early stages than from certified cases. In delusional insanity the disease has often made considerable progress before the relatives notice that something is wrong. Melancholics, as they recover, are possibly more capable of giving a useful account of their illness than at the onset of the disease. Primary dementia frequently commences with a period of confusion, during which it is impossible to get into touch with the patient at all. Certified cases are more likely to be true to type than early ones, in which mistakes in the diagnosis are more easily made ; therefore results of research into such cases would have more significance.

Dr. Suttie is not alone in his desire to see the psychoses looked upon as " failures or distortions of social adaptation." Dr. Brock considers it a mistake to look upon the neurotic's psyche first and foremost and to consider his environment a matter of secondary importance. Perhaps it is because the patient's psyche has not been given more time and consideration in the past that we have not solved the riddle of the psychoses. Certain " vogues " were mentioned by Dr. Suttie—including heredity, endocrine defect or unbalance, basal metabolism ratio disturbance, auto-intoxication, etc.—none of which, he says, were derived from clinical study. Perhaps too much has been claimed by these theories, but one cannot ignore the fact that in any mental hospital numerous cases could be demonstrated which substantiate their claims. They probably did originate from clinical observation, and were lines of research which were practical, as they did not call for uninterrupted attention, but could be investigated in the doctor's spare time. None of these factors are accepted as the primary cause of the psychoses, but they are recognized as important predisposing causes.

A study of the patient's environment, of modes of rearing, of domestic and social relationships, etc., is called for by Dr. Suttie. Probably this would add to our knowledge of the factors concerned with the onset of mental disease, but would it solve the riddle in question and not merely start yet another " vogue "? Supposing the child of an unintelligent farm labourer shows considerable promise at school ; we are asked to decide whether or not the child should be given the opportunity of a higher education. Our decision would be arrived at by consideration of the child's heredity, physical health and make up, etc. Is a

knowledge of the child's environment and its reactions to it going to enable us to be quite sure that he will be a success and not simply end by swelling the gradually increasing number of primary dements in our mental hospitals?

I feel certain that the solution of our riddle will be found in the patient who set it, not in his environment. Dr. Suttie would not express regret at having to cease the investigation of his case of paranoia, nor would he have spent 1,000 hours on it if he had not felt the same. Reason (1) (*b*) also suggests that he thinks the patient's psyche should get more attention. Special qualifications and much time would be required for this work. This brings us to the main reason why this riddle has not yet been solved—namely, expense.

It would be quite unwise to expect the committees of our mental hospitals to cater for research. It is their duty to the ratepayer to keep their expenditure as low as possible within limits. If research could be carried on outside the mental hospitals as is suggested it signifies the presence of the necessary funds. Could these not be more satisfactorily used inside these hospitals? They might be used to offer sufficiently attractive salaries to experienced and qualified men to devote their full time to research. More investigations on the lines of objective psychology and psycho-analysis are required. Such posts would enable the holder to carry on his work unhampered by the routine work at present done by the medical officers. I feel certain the hospital committees would help to reduce the expense incurred by such posts by supplying the necessary accommodation. General hospitals, supported by public subscription, are given to freely and liberally, on the whole, by the populace. Is the layman still too prejudiced against mental disease to be willing to give similar support to a free mental hospital? Such a hospital, run mainly for the purpose of research, would be one way out of the difficulties we are up against in our attempt to solve the riddle of the psychoses.—I am, etc.,

A. J. BAIN, M.B., CH.B., D.P.M.

West Riding Mental Hospital,
Wakefield, March 23rd.

The Climacteric

SIR,—I heartily subscribe to Dr. Bellamy's opinion (March 23rd, p. 618) that efficient treatment of disorders occurring at the climacteric is an urgent matter: as he rightly says, these troubles entail very real suffering. But I cannot so unreservedly accept his view that "we are all aware that endocrine dysfunction is at the bottom of the trouble," especially since he includes here disturbance in the mental field. It is true that the body affects the mind, and that the physical changes at this period of a woman's life must have their bearing on her personality as a whole. But it is not only what happens to an individual—and here organic disorder may be included—but the way in which she reacts that is of importance. The mind affects the body, and for some women the stress of the climacteric may be not the primary cause, but merely the spark which fires the train of which personality difficulties, until now latent, had earlier laid the foundation.

To ascribe without further examination any pathological mental phenomena occurring at this time to an endocrine origin would therefore seem to be unwise, even superficially. It overlooks the platitude that the existence of a physical abnormality does not always mean that the symptoms complained of are due to this anomaly. Even where endocrine disorder is associated with a given mental state there is not necessarily a simple causal relation between them. Rowe and Pollock, who examined a series of 4,000 patients with recognizable signs of endocrine disturbance, found that personality changes frequently did accompany the onset and progress of the malady, yet only one in sixteen gave evidence of recognizable mental abnormality. It is true that the cretin, to whom Dr. Bellamy refers, forms a well-defined physical and mental type. But is this condition really analogous to the climacteric, which is, after all, ultimately a physiological process through which many women pass without difficulty? In fact, Jones and Minogue found, when plotting the incidence of insanity in women on the basis of age, that a slight drop occurs after the age of 40, and the following plateau lasts until the age of 65, when there is again a rise.

Since, therefore, in dealing with these cases I consider that the personality as a whole should be taken into account, I do not agree with Dr. Bellamy that psychotherapy is unscientific in suitable cases—it may, in fact, be the best in some. But the method to which I refer is not, as Dr. Bellamy says, "no more than explanation of the cause of their troubles, encouragement, occupational treatment, or, finally, incarceration in a mental home"—and explanation for Dr. Bellamy would apparently merely mean a discourse on endocrine imbalance. The recent advances in psychotherapeutic methods cannot be neglected, and though many physicians consider that deep psychological investigation is inadvisable after the age of 50, analysis can be usefully employed in certain of these cases. Why, therefore, should the climacteric patient also not benefit?—I am, etc.,

London, W.1, March 25th. HILDA WEBER, M.D.

Routine Paraldehyde

SIR,—I have read Dr. J. W. Fisher's letter (*Journal*, March 30th, p. 677) with considerable interest, and while agreeing that the use of paraldehyde has the drawback of its unpleasantly distinctive odour, I feel that sight should not be lost of the fact that it is still one of the safest hypnotics. I am thoroughly aware of the temptation to issue the "nightly" draught, but as far as my own experience and practice goes this method of treatment is falling into disuse. As the idea of the "mental hospital" supersedes that of the "asylum," many changes of attitude are occurring, and one is that attention to the individual, in place of routine, treatment is becoming more and more of paramount importance.

The keystone of treatment to be effective in the early case is surely *Honesty*. However bitter reality and truth may be, they still are the positives. Much mental illness is the outcome of the inability to face them rationally. Treatment does not consist in pandering or "humouring." It is perturbing, therefore, to read as an alternative to the use of paraldehyde that of a nightly tea or soup into which drugs can be "slipped" when occasion demands, and with the aim of obviating drug consciousness. The practice of administering drugs surreptitiously in food has always appeared to me to be a particularly unpleasant one, and to have become even more undesirable with progress in mental treatment. Again, one of the chief aids to treatment lies in gaining the confidence of the patient, particularly the voluntary patient (if one must particularize), and for this reason the method suggested would seem apt to prove most destructive to its development. It is one thing to administer a powder, followed by a sip of water or warm milk to wash it down; it is rational to attempt to mask unpleasant flavours with suitable adjuvants (in either case the patient is aware that "medicine" has been taken); but it is quite another thing to place that powder in tea or soup or other common food to be taken unawares. Surely the self-proclaiming "routine paraldehyde" is infinitely preferable to routine soup which may have that little *extra plus*!

Furthermore, delusions centred round the idea that food is poisoned are not uncommon on the part of patients. I have found in a considerable number of cases that these ideas are founded upon almost imperceptible, and unrealized, changes in the sense of taste or smell. The "taste in the mouth" which accompanies the constipated state (an extremely common one in psychotics) furnishes an example. Are we not likely to foster the development of these false ideas if we "slip in" drugs unawares? Many of the delusions of the type I cite can be, and ought to be dispelled by explanation, but who would believe our explanations if we were found out in the adoption of what, after all, seems akin to the poisoner's art? By all means let us get rid of the "asylum smell," whether due to paraldehyde or to insufficiently opening windows, etc., but let us substitute the odour of trust.—I am, etc.,

Isle of Wight County Mental Hospital, March 30th. CHARLES DAVIES-JONES.

Medical Causes of Sudden Death

SIR,—The report in the *Journal* of March 30th of the interesting paper read by Professor J. Henry Dible before the Liverpool Medical Institution on the "Medical Causes of Sudden Death" prompts me to add another cause to the list given. I refer to the sudden and unexpected death which occurs not uncommonly in the course of the various types of Bright's disease, and in cases of prolonged anuria, whether associated with nephritis or not. I am unable at the moment to give statistics regarding the prevalence of this condition, as my investigations on the matter are not yet complete. Evidence of the fact, however, will be found in the writings of Dickinson, Roberts, West, Purdy, Herringham, Horder, and others, though the subject has not received the recognition which its practical importance merits. I do not, of course, refer to cases in which coronary or valvular disease is also found at necropsy. The immediate cause of death in many cases appears to be a toxic myocarditis induced by, or at least associated with, a considerable degree of azotaemia. In other cases it occurs in the absence of either marked nitrogen retention or hypertension. Sometimes death is preceded for some hours or days by occasional dropped beats, though in several instances an electrocardiogram revealed no obvious or characteristic abnormalities.

On several occasions, especially in cases of chronic anuria, I have been impressed by the fact that death ensued within a few hours of the patient asserting that he was going to die, although clinically there appeared to be no special reason to apprehend his immediate decease. In two of my patients death occurred during defaecation, and in two others shortly after a meal. It is clearly desirable that the possibility of sudden and unexpected death in such cases should be more widely known, and it is perhaps wise to mention it to the relatives. The risk should also be borne in mind when treating these disorders, as otherwise the treatment employed will almost certainly be held responsible.—I am, etc.,

London, W.1, March 31st. A. ARNOLD OSMAN.

Local Analgesia in Gynaecology

SIR,—In his interesting article in your issue of March 30th Mr. A. A. Davis makes two statements which cannot, I think, be substantiated in their entirety.

The first is that with local analgesia "there is practically no mortality." In an exhaustive survey[1] of nearly 50,000 unselected and consecutive operations in America performed under every variety of general and local anaesthesia, it was shown that the immediate death rate under local methods (0.001301 per cent.) was more than double that under general anaesthesia (0.000614 per cent.). One cannot make too much of statistics, as they are notoriously unreliable. Furthermore, the actual cause of death is not analysed, and it is possible that, on the average, local techniques were used on patients in worse condition than was general anaesthesia, but, nevertheless, these figures are suggestive.

Again, from a careful study of 21,000 case reports, a Continental surgeon[2] gives it as his considered opinion that the adoption of local analgesia does not in itself diminish the operative mortality rate. This is the more remarkable when one considers how general anaesthesia on the Continent has lagged behind that practised in this country. The writer, in his limited experience, has first-hand knowledge of two deaths which occurred actually during the injection of an infiltration analgesia.

Mr. Davis also states that "local healing is accelerated by the extra gentleness required of the operator by the avoidance of the painful effects of undue traction." Operative gentleness is undoubtedly beneficial and can be practised under any form of anaesthesia. There is, however, no doubt that while sterile isotonic anaesthetic solutions should not normally give rise to tissue irritation, extensive sloughing can occur in debilitated patients and those suffering from vasomotor and trophic lesions. Fatal results from this cause are not so uncommon as one might suppose, and reference to them was made last month by Dr. Gerald Slot[3] in his paper on anaesthetic fatalities. The risk of injecting solutions into infected tissues is, of course, well known.—I am, etc.,

London, N.W.8, March 29th. C. LANGTON HEWER.

Sterilization of Women: The Legal Position

SIR,—The interesting letter from Mr. Cecil Binney in your issue of March 30th confirms the opinion which I ventured with great diffidence to express at the Royal Society of Medicine—that there is considerable confusion of thought on the present legal aspect of sterilization. It must be recalled that only the legality of *eugenic voluntary* sterilization is at issue, for no one doubts that the sterilization of mental defectives or purely frivolous sterilizations are illegal. Mr. Binney, however, alleges that the consent of the patient to a eugenic sterilization would be valueless, as it would be "contrary to general principles" for the Courts "to accept an indirect benefit to the community as an excuse for doing an act prima facie wrongful." With respect, I think the point is incorrectly stated. A woman who has been competently advised that she is likely to bear haemophilic or mentally defective or deaf children, and wishes to marry, has at least as much to gain personally from the operation as would the "community." Further, I would urge that she and her surgeon have at least as good a defence as if she were getting her face lifted for aesthetic reasons.

Mr. Binney then makes confusion worse confounded in his last sentence, wherein he truly but irrelevantly states that it would be no defence to a charge of stealing whisky that the defendant reasonably believed that the prosecutor would be better without it. He has surely forgotten that in voluntary sterilization (which alone was under discussion) the patient has consented to the deprivation, whereas the victim of larceny *ex hypothesi* has not. A more correct analogy, if he wants one, would be the case (which occurred many years ago) of the teetotal peeress who inherited a cellar of valuable wines, and

[1] Bortone, F.: *Anesthesia and Analgesia*, November-December, 1932, p. 256.

[2] Bazy, L.: *Bull. et Mém. Soc. Nat. de Chir.*, December 17th, 1932, p. 1523.

[3] *Proc. Roy. Soc. Med.*, Anaesthetic Section, February 1st, 1935.

ordered her butler to pour them down the drain under the sincere belief that alcohol was poison. No one has ever contended that this act, though shocking to all right-minded persons, was in any way illegal.—I am, etc.,

London, W.C., March 31st. LETITIA FAIRFIELD.

SIR,—May I crave space for a few observations on Mr. Cecil Binney's letter in your issue of March 30th? The point taken by Dr. Letitia Fairfield in the discussion before the Royal Society of Medicine on February 15th (quoted in your issue of February 23rd), and referred to in my letter in your issue of March 16th, was read by me as relating to *voluntary* sterilization. And in my letter I confined myself to such. Therefore the instance quoted by Mr. Binney of the theft of whisky and the plea of the defendant that its owner would be better without it, seems scarcely analogous, unless, indeed, the owner had consented to its removal, in which case it would not, of course, be theft at all!

But the whole subject is a serious one, and goes deep. It concerns the race and posterity, including the possible acts of the latter, who in many cases are in the criminal courts still held legally responsible, and punishable therefor.—I am, etc.,

Lincoln's Inn, April 1st. W. H. STOKER.

Intravenous Olive Oil Emulsion

SIR,—I should like to draw attention to certain points in your annotation upon the above subject in the *Journal* of March 30th (p. 653). Your article states: "No indication that olive oil emulsions were being administered by intravenous injection for therapeutic purposes was given until the appearance of the paper on which the recent accounts in the daily press were based." You were good enough to publish last year (*British Medical Journal*, 1934, i, 558) a letter that now appears to have been overlooked. In it Dr. Walsh and I said: "We are investigating the use of emulsions intravenously in toxaemia of infective, biochemical, and traumatic origin in adults and in infants, and our results, which will shortly be published, are distinctly encouraging."

Your article also says: "The other paper in this volume [*Technical Aspects of Emulsions*] are concerned with purely industrial and engineering matters . . ." It should, I think, be added that the volume covers all the current problems concerned with the preparation and properties of emulsions in all fields of work. Further, the conference on emulsions, the proceedings of which this book contains, was held at University College under the chairmanship of Professor Donnan, F.R.S. Whilst it is true that this conference was primarily concerned with the physiological and technical aspects of emulsions, it is nevertheless true that a number of distinguished scientists were present, and that a number of members of the medical profession had been invited at my personal request.

We believe our problem to be a complex physical chemical one, and we felt that the opportunity for criticism from the eminent physical chemists who were present at that conference was one not to be lost, and that it had to be taken in the best interests of medical science. That our paper should have received such widespread publicity is undoubtedly regrettable, and on this point I need do no more than refer to the disclaimer you have already published from me. I should add that Dr. Walsh left London some two months ago for Australia.—I am, etc.,

ALASTAIR C. FRAZER.

St. Mary's Hospital Medical School,
April 1st.

Cancer and Causation

SIR,—In the *Journal* of March 30th Professor G. W. Nicholson implies that what he describes as "the mass of contradiction and the confusion of ideas that represent the cancer problem" are due to a misunderstanding of "causation" as applied to the phenomena of life. Nevertheless he leaves one not a little bemused as to what he himself means by this word. "The *cause* of a phenomenon is, of course," he says, "the factor, or factors, necessary for its being." But is this so? Carbon, hydrogen, oxygen, and nitrogen are material factors necessary for "the being" of a guncotton explosion, but they are not the cause of the explosion, which is the dynamic factor represented by a wave-impulse generated by a fulminate of mercury detonation. Every condition is obviously an effect that is the last term in a cause–effect series stretching back into infinity. A correct conception of the genesis of a malignant growth can only be gained when we are cognizant of each link in the cause–effect chain of stimuli and somatic reactions taking place within a limited life-period of the individual. In such chain, while we *may* meet with accessory, fortuitous, external stimuli—tar, soot, pipe-stem, or other "irritant"—we shall *certainly* have to reckon with diverse stimuli originating within the organism. Further, in the consideration of these latter and more important causes, discrimination will have to be made between principal and subordinate causes, and especially between preparatory and precipitating causes.

Professor Nicholson gives it out as his opinion that Mr. Morley Roberts was the first in the field when, fifteen years ago, he applied the biological conception of causation to cancer. That Dr. (later Sir) Archdall Reid anticipated Mr. Roberts by practically a quarter of a century is known to all who are conversant with his writings, notably with the thesis he set forth in *The Present Evolution of Man*, published in 1896. In connexion with a suggestion from Sir Henry Butlin concerning the origin of cancer, I called attention, in this *Journal*, in a letter dated December 2nd, 1911, to Reid's reversion-to-embryonic-cell-type theory, and quoted the following excerpt from his book, of which, perhaps, in fairness to the late Sir Archdall, you will permit repetition.

"Sometimes, though rarely, this or that cell-descendant of the germ reverts to the very remote unicellular ancestral type, in so far that it does not multiply at a rate bearing a definite proportion to the rates of multiplication of its other co-descendants of the germ-cell, but at a rate that has no definite proportion to them, and is only proportionate to its supply of nutriment and powers of assimilation. There then results the morbid condition which is known as a tumour (cancer), in which the cell-descendants of a cell which has so reverted to the remote ancestral type form a more or less spherical mass which performs no definite functions beneficial to the whole mass of the cell-community, but is rather a deadly encumbrance."

—I am, etc.,

CHARLES M. BEADNELL,
Surgeon Rear-Admiral.

Ringwood, Hants, March 30th.

Vegetative Balance and Chronic Disease

SIR,—In the *Journal* of March 30th appear two letters, one by Dr. H. W. Barber on "The Nature of Acidosis," the other by Dr. J. B. Christopherson on "Chronic Respiratory Disorders and the Autonomic System," which at first sight may appear quite unrelated to one another, but which, I submit, taken together, contain the essence of the aetiology of all chronic disease.

Barber contends that seborrhoea may be found in two distinct classes of patients: the fair, robust, and active individuals; and in the dark, pallid, thick-skinned, hypotonic

type of person—that in the former there exists an acidosis with an ammonia-combined free acid ratio in the urine which is high, and in the latter an alkalosis with a low ratio of combined to free acid. That is to say, that in a disturbance in the acid-base balance of the tissues, which may well be compensated completely so far as the blood *p*H is concerned, but in which urine analysis will show a disturbed metabolism in either direction, skin lesions may arise, which can successfully be treated by correcting the imbalance. This suggests that seborrhoea is not so much a disease entity as a symptom of a constitutional upset.

Now the two types mentioned by Barber are outstanding examples of differences in constitution depending upon vegetative balance as visualized by Krause and Zondek, in which not only is the vegetative nervous system disturbed, but which depend also upon changes in the amount of H and OH ions ; on the proportion of the ions of the divalent calcium to those of the monovalent potassium ; on hormonic influences, chiefly of the thyro-adrenal system ; and on colloidal disequilibrium. Here we get a connexion between the thought underlying Barber's letter and that of Christopherson, who contends that such conditions as asthma and chronic bronchitis are essentially due to autonomic dysfunction, and thus become, once again, not diseases, but symptoms of some constitutional defect.

I have for years contended that dental caries and pyorrhoea come into this same aetiological category ; that they depend primarily upon vegetative disturbance ; the former in the direction of sympathetic excess an excess of H ions (acidosis), an excess of Ca over K, an excess of thyro-adrenal activity, and a tendency to a break-up of the colloidal particles of the plasma (dehydration) ; whereas pyorrhoea depends upon a disturbance in the opposite direction, upon increased vagal tone (parasympathetic excess), an increase in OH ions, an overbalance of K as against Ca, a diminution of thyro-adrenal action, and a hydration of the colloidal protein particles of the plasma. Thus again these become symptoms and not diseases. A biochemical investigation of patients showing these dental lesions in an aggravated form upholds my suggestions, and if to this is added an ultra-microscopical examination of the blood plasma, as advocated by J. E. R. McDonagh, it becomes evident that here we have the *fons et origo* of dental lesions.

If this approach to the "nature of disease" is considered it will be seen how enormously medicine is simplified: hardly any of the conditions which to-day are dignified with the name of disease are, in reality, disease entities at all: many of them are but the symptoms of disturbed vegetative function, others are but the end-points of disease—as Christopherson contends are emphysema and bronchiectasis. Disease—that is, a departure from perfect health—becomes essentially and simply vegetative imbalance in the one or the other direction, which, in fact, explains the well-recognized antagonism of many diseases.

Furthermore, as soon as we realize this important fact, we are in a position to appreciate the very beginnings of disease, and to start preventive treatment before the end-points have developed ; for these changes in ionic balance, in hormonal function, in colloidal equilibrium, all of which are interrelated and interdependent, are diagnosable by comparatively simple tests, and may be corrected by a number of methods. Whether we approach the problem with Barber by way of the acid-base balance, by stressing the Ca/K ratio, via the endocrine-autonomic system, or through the chemico-physical state of the colloidal particles of the plasma as does McDonagh, is immaterial, as the ultimate object and the ultimate result will be the same. If we appreciate that immunity, in so far at any rate as chronic infection is concerned, is a matter of vegetative balance and not of the development of specific antibodies, then what Christopherson calls the "bacterial heresy" ceases to trouble, and focal infection takes its proper place in aetiology as but another consequence of inefficient vegetative function.

If, as suggested by Hahne, and more recently by McDonagh, the vitamins act, not by virtue of their chemical composition, but physically upon the colloidal particles of the plasma, hydrating or dehydrating these as the case may be and thus maintaining or disturbing vegetative balance, the deficiency diseases themselves fall into line. Again, if we consider such work as that of Cannon and Crile on the effects of emotional disturbance upon the thyro-adrenal system, and through this upon the vegetative system, the place of psychology in medicine is explainable. Thus, not only do we arrive at a point where we can separate diseases from symptoms, and commence treatment before organic change has taken place in organs and tissues, but the old classifications of disease according to the organs or tissues affected, which Christopherson laments, disappear and become meaningless, and specialism, in medicine if not in surgery, so condemned by Mackenzie as being the obstacle to the advance of the knowledge of the beginnings of disease, ceases to exist. Medicine becomes one unified whole.—I am, etc.,

Bournemouth, March 31st. F. W. BRODERICK.

The Osteopaths Bill

SIR,—About a month ago you did me the honour of printing a letter of mine in the *Journal*, drawing attention to the danger to the community of the clause in the above Bill which proposed to grant to osteopaths the power of death certification. I have now been informed that the promoters of the Bill have dropped that clause. Thereby the nation has been saved from a great peril, and we may perhaps venture to congratulate the osteopaths upon their (worldly) wisdom, and ourselves upon our timely and apparently decisive intervention.

But that power or privilege having now been set aside, for the time being at all events, we may well ask what is left to justify a special Act of Parliament, creating a new kind of medical practitioner to the confusion and disturbance of the lieges and the law of the land. It would appear as if we were about to see something like a novel sort of dentist. And the apparent resemblance to the dentist may yet be made an argument in favour of the Bill. But note the difference: dentist's work is confined to a definite region and set of tasks, and it is based ultimately upon rational pathology. The osteopath, on the other hand, claims the whole body for his own, and his therapy is based upon what we can only call a fantastic pathology.

Before I withdraw from what is to me an unusual incursion into medical politics, I should like to call attention to one great truth appropriate to the occasion that we doctors seem to be forgetting. I enunciate it now and commend it to the consideration of our leaders. It is this: our knowledge of medical history and human nature alike shows us all too plainly that no cure or series of cures can alone substantiate the truth of any pathology. Are not the lives of the Saints—Christian, Mohammedan, Buddhist—full of such cures? Nay, even the annals of the witch doctors of Central Africa, of the sorcerers of New Guinea, of the shamans of Siberia, are equally rich in such miracles. Yet I do not suppose that the House of Lords committee would be at all inclined to treat them seriously. But why not? They are of the same order of events as the osteopathic cures we have been hearing so much about.—I am, etc.,

London, W.1, March 29th. DAN MCKENZIE.

Voluntary Aid in Diabetic Clinics

SIR,—The Diabetic Clinic of the Royal Portsmouth Hospital is fortunate in that its work has appealed to a number of voluntary workers. Actually these workers, who are recognized members of the clinic, are commandants or assistant commandants of voluntary aid detachments of the British Red Cross, and the work they have accepted is to visit some of the patients in their homes and to furnish written reports of their findings to the clinic. A call from such a visitor is offered to the patient in the first place, with the explanation that the visitor will come as a friend with whom difficulties regarding diet or other treatment may be discussed. As a fact, these visits are proving popular, and are of real use in those cases where progress remains unsatisfactory. The visitors are trained in the technique of the hypodermic syringe and insulin bottle, diets are explained to them, they are taught urine testing, and are especially taught to detect an early or late ketosis.—I am, etc.,

H. FARNCOMBE,
Honorary Physician, Royal Portsmouth Hospital.

Portsmouth, March 25th.

Treatment of Hammer-toe

SIR,—I read with interest Mr. Creer's description of an ingenious and apparently satisfactory splint for post-operative fixation of hammer-toe (*Journal*, March 16th, p. 527), but I cannot help feeling that anyone who sets out to review the operative treatment of hammer-toe and omits Mr. Higgs's spike operation must be limiting his outlook unduly.

This operation, which comprises inserting the proximal phalanx, sharpened to a point, into a hole in the distal phalanx fashioned to receive it, has several advantages over all others. When properly performed no splint is necessary. The fragments cannot be displaced unless pulled apart with sufficient force to tear out the sutures. Flexion alone will not do this. When the sutures are removed, in eight or ten days, the patient can walk painlessly. In my experience fibrous union does not occur. The only disadvantage is that it takes a little longer to perform. It is, moreover, in accordance with the sound orthopaedic principle of "putting the splint inside."—I am, etc.,

Norwich, March 27th. H. A. BRITTAIN, M.CH., F.R.C.S.

Post-mortem Examinations

SIR,—Individual efforts to popularize post-mortem examinations—for example, Dr. C. O. Hawthorne's proposals mentioned in his letter in your issue of March 23rd (p. 616)—have not found the deserved appreciation, and it still remains true (to quote an essay published by Dr. Hawthorne in 1928) that "the practice of post-mortem examination repels rather than invites attention, and the general and conventional attitude is to ignore it." Many practitioners find the subject unsuitable for discussion with patients, especially during a depressing illness. It appears to me, therefore, that these problems call for corporate effort on broadest lines.

A Society for the Investigation of Death Causes, uniting medical men with lawyers, scientists, educationists, and other laymen, could deal more effectively with obsolete prejudices and misapprehensions and succeed, perhaps, in the not too distant future in establishing necropsies as a generally accepted modern funeral rite. Public health authorities should realize the importance of providing an efficient necropsy service conveniently available to every practitioner. The Medical Research Council might be interested in obtaining a comprehensive necropsy register for easy reference and statistical purposes through the filing of reports on every necropsy carried out in this country.—I am, etc.,

London, W.1, March 24th. F. E. LOEWY.

Histamine Ionization

SIR,—In the March issue of the *Prescriber* Dr. G. L. Kerr Pringle, in discussing accessory treatment at spas, makes the following statement under the heading of ionization:

"The histamine ion has also been used in these cases [the arthritides], but it is very probable that the results are due to the prolonged galvanism and not to electrolysis or any particular drug."

This I cannot possibly agree with. In the first place the electrical application of histamine is extremely rarely "prolonged": in fact, many cases react completely in about two minutes, and will at the end of that time display all the signs and symptoms of the beneficent action of histamine—loss of pain, increased movement, etc.

I think no one would expect such a result to follow the passing of a galvanic current for an equal time; and, secondly, I know from my own experience that the use of the galvanic current alone, for no matter how long a period, will never equal in results the effect produced by the addition of histamine.

At the time when the subject is receiving such wide attention I feel it is necessary to correct any statements which might tend to cause uncertainty regarding the worth of one of the most valuable of the newer treatments.—I am, etc.,

Droitwich Spa, March 19th. F. SEVERNE MACKENNA.

The Services

The Honours List issued on the occasion of H.R.H. the Duke of Gloucester's visit to Australia and New Zealand includes the names of Surgeon Commander A. W. Gunn, R.N., and Surgeon Lieutenant Commander L. Lockwood, R.A.N., as recipients of the M.V.O. (Fourth Class).

DEATHS IN THE SERVICES

Surgeon Captain Henry Erskine South, R.N. (retired), died in London on March 10th, aged 72. He was born in 1862, was educated at Charterhouse and at St. George's, and took the M.R.C.S. and L.S.A. in 1885. Entering the Navy soon after, he became Surgeon Commander on February 16th, 1901, and retired, with an honorary step in rank as Surgeon Captain, on November 11th, 1918. During the war he served on H.M.S. *Courageous*, and later as P.M.O. in Devonport Dockyard; and after the war on the medical staff of the Ministry of Pensions and on the Pensions Appeal Tribunal. He was the author of a book of naval sketches, *The Destroyers, and Other Stories of the R.N.* A former colleague writes: In each post under the Ministry of Pensions his work was rendered the more valuable by his long experience of Service conditions and the special gift he possessed of assessing the personality as well as the physical condition of the man under review. A man of outstanding appearance and physique, Captain South's looks belied his years, and his sudden death has come as a shock to all his friends. He leaves a widow and a daughter, to whom is extended the sincere sympathy of a wide circle.

Surgeon Major Septimus Jesse Goldsmith, Bombay Medical Service (retired), died at Cheltenham on March 3rd, aged 84. He was born on January 28th, 1851, the son of George Goldsmith, Esq., barrister, of Leyton, was educated at St. George's, and took the M.R.C.S. and L.R.C.P.Lond. in 1872. Entering the I.M.S. as assistant surgeon on April 1st, 1873, he became surgeon major after twelve years' service, and retired on March 4th, 1891. At the time of his death he was the senior living member of the Bombay Medical Service. There can be few men left now who entered as assistant surgeons, a rank abolished three months after he joined.

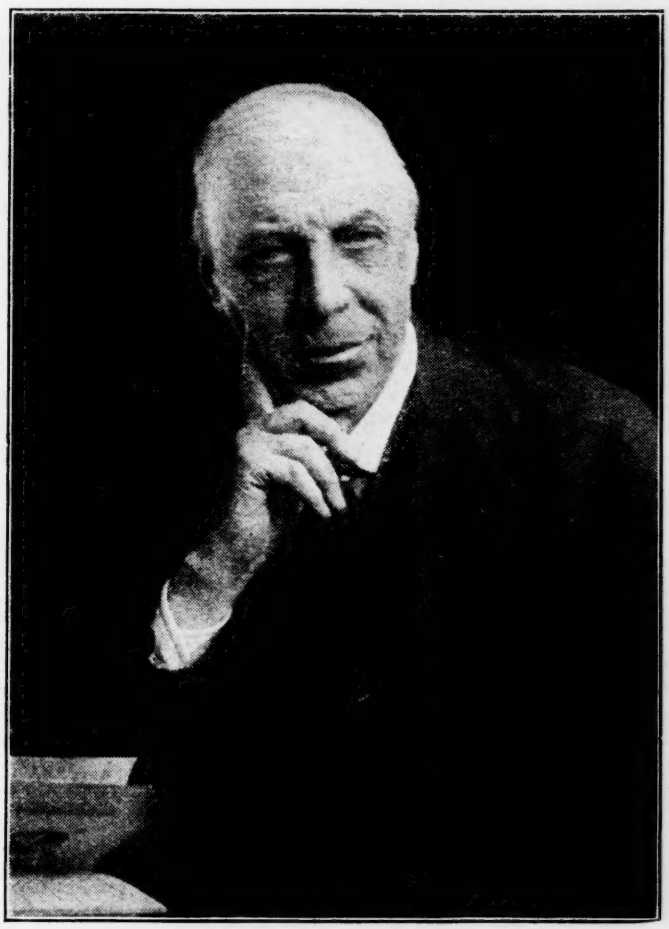
Obituary

SIR EDWARD SHARPEY-SCHAFER, F.R.S.

LL.D., M.D., D.Sc., F.R.C.P.Ed.

Emeritus Professor of Physiology in the University of Edinburgh

By the death of Sir Edward Sharpey-Schafer on March 29th, in his eighty-fifth year, the world loses one of its most distinguished physiologists, and one whose name is very widely known to the general public, for his method of giving artificial respiration in the prone position has been adopted by all those who have to do with ambulance work and teach the means of saving life.



E. Sharpey Schafer

Born in London on June 2nd, 1850, the son of James William Henry Schäfer of the free city of Hamburg and of Highgate, he was educated at Clewer House School and University College, London, where he came under the influence of William Sharpey, the physiologist, whose name he added to his own in 1918 to emphasize the fact that he was an Englishman by birth and education, at a time when anyone with a German name was regarded with suspicion and hostility by newspaper-inflamed ignorant people. Such action in his case should have been wholly unnecessary, for both his sons lost their lives in the war, one, I believe, when serving in a "Q" boat.

In 1873 the young Schafer became the first Sharpey scholar, and in 1874 was appointed assistant professor of physiology at University College when Sharpey resigned to be succeeded by Burdon-Sanderson. On the latter going, in 1883, to fill the newly created chair at Oxford, Schafer became Jodrell professor, and held this chair until 1899, when he was elected to the chair of physiology at Edinburgh University. While in London he also held the Fullerian professorship at the Royal Institution. From 1895 to 1900 Schafer acted as general secretary to the British Association, and was elected president in 1912. In 1923 he was president of the International Physiological Congress. As president of the British Association he gave an address which attracted wide attention, in that he claimed that life was due to no mysterious vital force, but to the action of chemico-physical laws. Confirmation of his views may be now found in the fact that modern physical research has shown that the atomic structure and interaction of the non-living is just as subtle and beyond the scope of ultimate explanation as that of living bodies. Many honours deservedly fell to Sharpey-Schafer. He was knighted in 1913, received honorary doctorates from Oxford, Cambridge, Aberdeen, St. Andrews, Dublin, McGill, Melbourne, the National University of Ireland, Berne, Gröningen, and Louvain, and was made an honorary F.R.C.P.Ed. He was elected a Fellow of the Royal Society in 1878 when 28 years old, served more than one term on the council, and was awarded a Royal Medal in 1902 and the Copley Medal (the highest honour of the society) in 1924. In 1897 he received the Baly Medal of the Royal College of Physicians of London, and in 1911 the de Cyen Prize from the Academy of Sciences of the Royal Institute of Bologna. He was invited to lecture at Johns Hopkins University, Baltimore, and the Leland Stanford University, and was made a corresponding member of many foreign and learned societies.

At the beginning of his career Schafer specialized in histology, and in particular carried out researches into the minute structure of the muscle fibre, using the remarkable wing muscle of insects to illustrate his views. He was author of *Essentials of Histology*, which has run through very many editions, and must have proved a steady and valuable minor source of income; he also dealt with minute anatomy in a scholarly volume published in the tenth edition of *Quain's Anatomy*. Having carried out, with Victor Horsley and others, many researches on cerebral localization, and detailed these in no fewer than twenty papers, he became, with George Oliver, the discoverer of the remarkable action on blood pressure which followed an injection of an extract of the suprarenal gland. Oliver had invented the arteriometer, an ingenious instrument by which he measured the diameter of the radial artery, and had made observations on patients, who, if I remember right, took suprarenal extract by the mouth, which led him to suggest the research to Schafer. The surprising results which they obtained on injection started a prolonged inquiry into the action of glandular hormones, and opened up a vast new field of research in physiology with important applications to clinical medicine. The sum of these researches was given by Sharpey-Schafer in his book *The Endocrine Organs*. One of his assistants, Professor Swale Vincent, became a distinguished worker on this line. Schafer also edited an *Advanced Textbook of Physiology*, to which many of the leading physiologists in this country contributed. In recent times, to commemorate the jubilee of the society, he put together a history of the Physiological Society, 1876–1926, and on his retirement from the Edinburgh chair the society held a dinner in his honour. He was then the only survivor of those nineteen persons who met in London on March 31st, 1876, and resolved to found that society. Schafer started, and edited up to his retirement,

the *Quarterly Journal of Experimental Physiology*, which has now reached its twenty-fifth volume.

His contributions to scientific journals were very numerous, as he continued actively engaged in research up to his retirement in 1933, when he was made Emeritus Professor. His past and present assistants then published in his honour a special volume of the *Quarterly Journal of Experimental Physiology*, composed of 29 papers on original researches carried out by themselves. In the preface to this volume it was stated that "Sharpey-Schafer's contributions to physiological science are stored in the libraries and utilized daily by research workers in further extending the boundaries of knowledge which he himself has done so much to advance." A special volume of this publication, bound in calf, was presented to Sharpey-Schafer, containing a vellum interleaf signed by all the contributors. This interleaf had to be sent round the world—to America, Cape Colony, New Zealand, China, etc.—to secure the signatures of those whom the master had taught, and who had gone out to fill chairs of physiology in many distant climes.

Sharpey-Schafer had four children, and is survived by one daughter and his second wife, Ethel Maud, daughter of J. H. Roberts, F.R.C.S. On his retirement, while still in remarkable vigour and good health, he underwent a severe operation, which unfortunately was followed by pneumonia. From this he never fully recovered, and remained frail up to his death. His long years of vigour were due in part to the excellent golf which he played on the famous links at North Berwick, where his charming house was situated.

For the foregoing memoir we are indebted to Sir LEONARD HILL, *who adds a personal appreciation*:

I was taught physiology by Sharpey-Schafer, and became, forty years ago, intimately acquainted with him on being appointed Sharpey scholar, and, later, assistant professor under him at University College, London. He was then in his prime, and lived at a house he had had built in beautiful surroundings at Croxley Green. How clearly I remember a walk taken with him and his four young and lovely children on a June Sunday, when a well-trained faculty for birds'-nesting enabled me to find nest after nest to the delight of the children and the amazement of their father. Alas! three of these children were to meet tragic deaths. Working in the physiology laboratory, then at University College, were Sidney Ringer, who used to come before breakfast to test mosquito larvae in saline solutions, Victor Horsley, Halliburton, Marcet, Sidney Martin, Rose Bradford, Ruffer, George Oliver, and Bayliss. The laboratory was small and comparatively ill equipped, but an immense amount of research was done. Schafer left his assistants free to carry on their work, ready to help when asked. I myself owed very much to his example and to his kindness and forbearance. As a teacher Schafer was accurate and precise, speaking with no rhetorical flourishes, but with a quiet and rather cold delivery.

In his triple capacity of researcher, teacher, and administrator he rendered invaluable services, first to University College and then to Edinburgh University. Passionately devoted though he was to his research work, he never allowed this to interfere with what he considered his duties to his students or to his University. His department at Edinburgh was, I am told, so efficiently organized that personal obtrusiveness was unnecessary, and although in the latter years he assumed rather an Olympian attitude in the eyes of the younger generation of students, yet one and all realized that he was not only available but all too willing to help them in their difficulties. A superb intolerance of intellectual dishonesty was combined in him with a kindly, generous tolerance of the inevitable slips and errors of his assistants and students. His intensely logical and inductive mind proved of the greatest assistance at University College, and, I understand, to the Edinburgh Senatus; it will be long ere his colleagues find a successor to whom they may appeal with more confidence for a judicial verdict on their deliberations. Many generations of students and assistants have carried away with them as one of their pleasantest memories those delightful summer parties at North Berwick, where Sir Edward and Lady Sharpey-Schafer proved to be the perfect host and hostess. It is in ways such as these that he remains endeared to those whom he taught and with whom he worked. His output of research work was extraordinary, and was carried on without diminution till over eighty years. This work stands as the testimony of his greatness.

L. H.

Sir Edward Sharpey-Schafer had been a member of the British Medical Association for the past fifty-five years. At the Annual Meeting in Liverpool in 1883 he was president of the Section of Anatomy and Physiology; at the London Meeting in 1895 he gave the Address in Physiology; and at the Edinburgh Meeting in 1927 he was president of the Section of Physiology and Biochemistry.

[The photograph reproduced is by Swan Watson, Edinburgh.]

SIR HORACE PINCHING, K.C.M.G.

Formerly Director-General, Egyptian Public Health Department

We regret to announce that Major Sir Horace Pinching, K.C.M.G., late R.A.M.C., died suddenly in Cairo on March 28th.

Horace Henderson Pinching was born at Gravesend on May 19th, 1857, the son of the late C. J. Pinching, J.P., of that town, was educated at Epsom College, Pembroke College, Oxford, and St. George's Hospital, graduated M.A.Oxford in 1882, and took the M.R.C.S., L.R.C.P. Lond. in 1883. Entering the Army as surgeon on February 2nd, 1884, he became major after twelve years' service, and retired on September 23rd, 1896. In March, 1888, he was seconded for service with the Egyptian Army; in September, 1894, he was transferred from the Army to the Sanitary Department, Egypt; in 1899 he was appointed Director-General of the Egyptian Public Health Department, and retired from that service in 1908, but made his home in Cairo.

He served in the Sudan in three campaigns: in the Nile expedition of 1884–5, medal with clasp and Khedive's bronze star; at the action of Giniss in 1885; and in the campaign of 1888–9, at the actions of Gamaizah and Toski, was mentioned in dispatches in the *London Gazette* of September 6th, 1889, and received two clasps and the fourth class of the Medjidieh. Besides the Medjidieh, he received the Order of the Star of Ethiopia, second class, in 1897, and the K.C.M.G. in 1902. In 1903 Sir Horace Pinching married Edmée Anna, daughter of Ernest Cramer of Pressy, Geneva, and had two daughters. He had been a member of the British Medical Association for more than forty-seven years.

G. E. ODDIN-TAYLOR, M.R.C.S.

Past President of the Natal Inland Branch

The leader of the medical profession in Pietermaritzburg, Natal, Dr. Gordon Ernest Oddin-Taylor, died in Grey's Hospital on March 15th.

Born in Norfolk sixty-one years ago, and a student of St. Thomas's Hospital, Oddin-Taylor qualified M.R.C.S. Eng., L.R.C.P.Lond. in 1896, was appointed house-surgeon at his own hospital, and went out to Pieter-

maritzburg during the Boer War. He had practised medicine in that town ever since. His bent was surgical, and for the past twenty-eight years he had been honorary surgeon to Grey's Hospital. He was loved and respected by his colleagues, by the nursing staff, and by his patients. His gentle and conservative manner and his keen dislike of anything savouring in the least of publicity served to stamp him as a practitioner of the finest type.

He had been a member of the British Medical Association for many years, and had served on the council of the Natal Inland Branch since its inception. In 1933 he was president of the Branch, and had previously served as the elected member of the Branch on the Federal Council of the Medical Association of South Africa. He was unanimously elected as the representative of the Branch for the Centenary Meeting of the Association held in London in 1932. He married a daughter of the late Mr. Dumas of Mount Edgecombe, Natal. Mrs. Oddin-Taylor survives him, together with two children, a son and a daughter.

Dr. JAMES CARDWELL GARDNER of Amersham, who died on March 25th at the age of 70, had been from his early youth prominent in athletic sports. Born in Cheshire of a Lancashire family he was educated at Rugby, where he won his first trophy at the age of 11, and later on the Public Schools shooting championship. Going up to Cambridge, he distinguished himself in rowing at Emmanuel, stroking the victorious Cambridge crews of 1888 and 1889, and again in 1890, when Oxford won. He stroked the winning crew for the Stewards' Cup at Henley in 1891 and 1894, as well as various Leander and Thames crews. He won the Colquhouns in 1886, the Diamonds in 1887, and the Wingfield Sculls in 1890. With Mr. S. D. Muttlebury he won the C.U.B.C. pairs and the silver goblets in 1889. Proceeding to St. George's Hospital he joined the Rugby team, and played also for the United Hospitals and the Midland Counties. He won many boxing prizes as a member of the Belsize and Thames Boxing Clubs. After graduating M.B., B.Ch. Camb. in 1894, he held the posts of house-physician and house-surgeon at St. George's Hospital, thence going to Amersham, where he soon built up a large general practice. He was for a time medical officer of health of the Amersham rural district, and soon became a distinguished figure in the social and public life of the country around. Twenty years ago he was made a justice of the peace for Buckinghamshire, and later was chairman of the local bench of magistrates. His versatility at sports continued to develop, notwithstanding his heavy professional and civic obligations; he played Association football, and captained the Amersham cricket team until he was 60. He was an original member of the Harewood Downs Golf Club, and his house contained numerous golf, swimming, tennis, fives, and cycle-racing trophies. A fine horseman, he was for some years secretary of the Old Berkeley Hunt, while latterly his favourite recreation was shooting. For one who was a keen and devoted general practitioner such an athletic record must be very hard to equal. Dr. Gardner joined the British Medical Association in 1895, and was a Fellow of the Society of Medical Officers of Health.

The following well-known foreign medical men have recently died: Dr. POIRIER of Antwerp, president of the Belgian Dermatological Society; Professor ACHILLE DE FABII, an eminent surgeon of Rome; Dr. ERWIN LIEK of Berlin, aged 57, author of a work translated into English under the title of *The Doctor's Mission*; Dr. BERNARD SPATZ, for forty-two years editor of the *Münchener medizinische Wochenschrift*; Dr. SOWINSKI, professor of dermatology at Warsaw; Dr. MIHAIL BUTOLANU, professor of operative surgery at Jassy, president of the Rumanian Society of Surgery; and Professor WILHELM KATTWINKEL of Partenkirchen, author of numerous works on sleeping sickness.

Medico-Legal

DAMAGES AWARDED AGAINST AN OSTEOPATH

In the King's Bench Division, before Mr. Justice Talbot and a jury, on March 13th and succeeding days, an action was heard in which Sheila M. R. Smith (suing through her father) and Mrs. Kathleen Smith, her mother, of Hampstead Road, N.W., sought to recover damages from Captain Gerald Lowry, a blind osteopath and masseur, of Upper Berkeley Street, W., for personal injuries suffered by the infant plaintiff and consequential loss by her mother due to negligence in treatment.

Mr. E. C. Kingsbury, in opening the plaintiff's case, said that Sheila, now aged 15, was a normal child, but at 5 years of age had rheumatic fever, and about four years later complained of pains in the leg, which were attributed to rheumatism. In August, 1931, Captain Lowry was consulted, and stated that the girl was suffering from dislocation of the left hip and twisted intestine, and, it was alleged, negligently and violently so twisted her left leg as to fracture the neck of the femur, with consequent permanent restriction of the left hip.

Mrs. Smith stated that Captain Lowry ridiculed the idea that the child had rheumatism. While he was manipulating, the child, who was under an anaesthetic, gave a scream, and a cracking was heard, which caused Captain Lowry to remark, "That was a funny sound." Afterwards the child was in great pain, but Captain Lowry assured her that this was due to tautness of the muscles after the operation. Next day, however, he sent a radiographer and a surgeon, and the latter declared that the child had a double fracture, and she was taken to King's College Hospital, and eventually to the Royal Waterloo Hospital, the whole treatment taking about a year. The witness denied that the child had to be carried into Captain Lowry's consulting room. Evidence was also given by the father, who said that before she saw Captain Lowry his daughter walked like any other child, and by Sheila Smith, who said that when Captain Lowry was ready to begin his treatment she was able to climb on the operating table by herself. Then, under the anaesthetic, something happened, she felt a pain in her back, and heard herself scream. A nurse employed by Captain Lowry at the material time said that during the manipulation a sound like cracking was heard, and she said, "I think you had better stop."

Mr. Noel Dudley Buxton, orthopaedic surgeon to King's College Hospital, said that the *x*-ray photographs taken after the manipulation showed a fracture of the neck of the left thigh. The girl was given treatment and placed in a plaster-of-Paris splint for three months. In cross-examination, Mr. Buxton was shown an *x*-ray photograph of the hips stated to have been taken a year before Captain Lowry's treatment; he said this revealed a displacement of the neck of the right hip-bone, and therefore the condition of that hip, for which he did a corrective operation in 1932, seven months after Captain Lowry's manipulation, could not have been caused by that manipulation. He agreed that a manipulation which would cause no injury to a normal child might fracture the thigh of a child suffering from displacement. If an *x*-ray photograph had been taken before the treatment it would have disclosed the dangers of manipulation to a man who knew his work.

Mr. G. O. Slade, for the defence, said that Captain Lowry would not allow him to call the anaesthetist whom he had employed, because an anaesthetist assisting an unqualified osteopath rendered himself liable to proceedings in the General Medical Council. Dr. Hazel Chodak Gregory, in evidence, said that the child was under her care in the Royal Free Hospital a year before the manipulation, when a waddling gait was very noticeable. She had her *x*-rayed, and came to the conclusion that she was suffering from displacement of the neck of the thigh-bone on both sides. Dr. Claude Gouldesborough, radiologist, said an *x*-ray photograph taken in 1930 showed a definite, though very early, displacement of the neck of the thigh-bone. Captain Lowry, in evidence, said that since he lost his sight in the war his sense of touch had developed wonderfully. He was the first blind man to qualify as a masseur from St. Dunstan's. He had practised osteopathy since 1925. On beginning the manipulation of

this child he discovered that it was not a case for manipulation, but for a surgeon, and told the mother so. Nothing occurred during the operation which led him to think that anything untoward had happened, and he denied some of the statements made by the mother. The child cried after the examination, but did not scream. It was untrue that he was perturbed about the case, though he sent a radiographer next day and also asked his wife to call and assist.

The jury returned a verdict in favour of Sheila Smith, to whom they awarded £1,500 damages for personal injuries, and judgement was entered to that effect. On the claim by Mrs. Smith for consequential financial loss judgement was entered for Captain Lowry, and a counter-claim by Captain Lowry for £7 10s., the balance of his fee of ten guineas, was dismissed.

A WARNING TO HOSPITALS

On March 21st, before Mr. Justice Horridge, a laundrymaid obtained £695 damages and costs against the West Ham Corporation for injuries to her hand caused at Goodmayes Mental Hospital by a calender machine, which she alleged was insecurely fenced. The corporation gave notice of appeal, and the case is therefore still *sub judice*, but certain aspects of it are not affected by the possibility of review.

Calender machines are installed in practically every mental hospital laundry—and for that matter in the laundries of nearly every institution of any size. Their function is to wring and iron clothes by the same process. A series of moving tapes feeds the wet clothes on to a steam-heated plate about 3½" in. in width, which curves downward away from the operator and guides the clothes in between two steam-heated rollers. The approach to the hot plate is guarded, to satisfy the Factory and Workshops Acts, by a swinging guard or screen; this is hinged along the top, and hangs down to within about half an inch of the moving tapes, extending across the whole width of the machine. Folded clothes will pass under it, but it is designed to prevent the workers' hands from entering with the clothes, and it is connected to the machinery in such a way that a movement of about three-quarters of an inch towards the rollers will stop the machine. In the present case the laundrymaid sustained her injuries in spite of the guard. According to the evidence, she noticed that a nightgown which she was feeding into the machine had one of its sleeves turned inside out. She thrust her hand into the sleeve in order to reverse it, but the cuff was already caught by the rollers; as they drew in the sleeve the stuff tightened round her hand and she could not release it. The guard seems to have stopped the machine according to plan, but nevertheless the girl's hand was drawn on to the hot plate—heated to a temperature well above boiling-point—and held there for about ten minutes until someone thought of cutting the sleeve of the nightgown. Her little finger was broken—possibly in the rollers—and she was badly burnt; an extensive amputation was necessary, and she suffered from residual infection of the bone. The reason for the failure of the safety mechanism appears to have been that the guard, instead of descending perpendicularly, curved inwards about two inches. Mr. F. A. Thomas, a consulting engineer, who gave evidence for the plaintiff, said that this was a usual practice. Moreover, even after the guard had worked the trip mechanism, the rollers were able to travel another inch and a quarter, owing to a certain amount of lag almost inseparable from the working of the brake mechanism.

The judge pointed out that in considering whether machinery is dangerous the contingency of carelessness on the part of the workman, and the frequency with which that contingency is likely to arise, must be taken into consideration. The hot plate, therefore, he considered, was not securely fenced against the danger which ought to be apprehended of workers getting their hands entangled in garments and burnt. He did not think that the laundrymaid had been negligent herself; she had never been told that if she folded the garments in a certain way her hand might be drawn in and injured, and her action in trying to reverse the sleeve was a natural one. The striking lesson to be drawn from the case seems to be that the makers and users of these machines have always regarded the rollers as the dangerous part, and not the fixed hot plate, which in the present case was the cause of the injury. Mr. Justice Horridge suggested that the remedy was to put the guard forward to the extent of at least two inches or more, so that in no circumstances could the worker's hand, even with a doubled-up garment, be drawn on to the hot plate. Mr. Thomas said that he had known a great many similar accidents. Those responsible for institutions would be well advised to turn their attention towards this potential source of liability.

Universities and Colleges

UNIVERSITY OF LONDON

CANCER HOSPITAL (FREE)

The following candidates have been approved at the examination indicated:

ACADEMIC POST-GRADUATE DIPLOMA IN MEDICAL RADIOLOGY.—*Part I:* F. M. Abeles, P. A. Flood, D. Hugo, E. G. Lynch, M. Mani, S. Nowell, T. C. Wasson.

VICTORIA UNIVERSITY OF MANCHESTER

Stella H. Brown, M.B., Ch.B., has been approved at the examination for the Diploma in Psychological Medicine (Part I).

UNIVERSITY OF LEEDS

The following candidates have been approved at the examinations indicated:

FINAL M.B., CH.B.—*Part I:* G. N. Blackburn, Olive M. Callow, J. C. Coates, Joan M. Coward, W. Davidson, W. G. France, M. Headlam, W. Hobson, K. K. Hussain, R. L. Lamming, D. M. Leiberman, D. J. McCandlish, S. Mattison, W. S. A. Oakes, R. Petticrew, G. Quayle, S. H. Samuel, Winnie Shaw, C. E. Stuart, C. W. Ward. *Part II:* Mary Barraclough, T. M. Boyle, Julia M. Brown, D. F. Cameron, G. A. Collinson, W. G. L. Flather, Irene Haslegrave, E. H. Kitching, E. F. Lai, H. T. Levi, A. E. Penn, F. Reid, H. Silman, H. E. Steward, C. C. N. Vass. *Part III:* *E. H. Kitching, †G. A. Collinson, Mary Barraclough, T. M. Boyle, Julia M. Brown, D. F. Cameron, W. G. L. Flather, Irene Haslegrave, E. F. Lai, H. T. Levi, A. E. Penn, L. Rabinovitch, F. Reid, H. R. Rollin, H. Silman, H. E. Steward, D. Thackray, C. C. N. Vass.

* First-class honours. † Second-class honours.

The following awards have been made. *Infirmary Scholarship and Littlewood Prize:* C. Pickard. *Hardwick Prize:* Divided between J. F. Goodall and E. H. Kitching. *McGill Prize:* F. R. R. Martin. *Scattergood Prize and Edward Ward Memorial Prize:* E. H. Kitching.

UNIVERSITY OF LIVERPOOL

At an honorary graduation ceremony next month the degree of LL.D. will be conferred upon Mr. Charles Thurstan Holland, Ch.M., F.R.C.S., formerly lecturer on radiology in the University; and Professor Arthur Harden, D.Sc., F.R.S., late head of the Biochemical Department of the Lister Institute.

UNIVERSITY OF DUBLIN

TRINITY COLLEGE

The following candidates have been approved at the examinations indicated:

FINAL MEDICAL EXAMINATION.—*Part I (Materia Medica and Therapeutics, Pathology, and Bacteriology):* W. Hayes, B. P. P. Berney, G. C. Blackham, G. L. Daly, G. C. Retz, R. L. Bustard, W. J. G. Warwick, H. S. Boyce, F. Keane, J. G. Cunningham, T. S. Agnew, W. S. M'Bride. *Part II:* M.B.: J. N. P. Moore, M. Marmelstein, P. Citron, J. T. Wellwood, J. R. Shapiro, J. J. Talbot, L. Fridjhon. Under old regulations: H. G. Ellerker. B.Ch.: D. S. Quill, J. Sorett, T. A. Cunningham, A. F. J. Delany, C. M. T. Adamson, D. J. Bradley, R. I. Shier, Charlotte F. Pike, H. M. Glover. B.A.O.: B. E. O'Brien (passed on high marks), J. A. Hamilton, G. J. Dixon, G. H. B. Roberts, A. F. H. Keatinge, H. J. Anderson, S. B. Sachs, B. Lapedus, J. C. Roux, D. J. Hayes, H. B. Sutton, F. T. P. Bergin, D. H. T. Duggan, N. G. Farrell, B. M. O'Sullivan, H. M. Glover, W. Charney, E. Cowan. *Diploma in Gynaecology and Obstetrics:* T. K. Ghosh, A. K. Acharya.

D.P.H.—*Part I:* G. A. Miller, D. B. Bradshaw, J. K. Lavery, R. L. G. Proctor, T. J. O'Sullivan, P. G. Daly.

BRITISH COLLEGE OF OBSTETRICIANS AND GYNAECOLOGISTS

The following have been awarded the Diploma of the College:

A. K. Acharya, Annie F. Allport, W. N. Brown, Helen R. Buck, Dora Davies, Muriel H. Deem, S. K. Hutton, C. N. Jeffries, Mary K. Lawlor, Barbara H. Mitchell, J. Morrissey, E. B. Murrell, J. L. Parker, Helen I. Robertson, Isabella Taylor, S. W. Wright.

Medical Notes in Parliament

[FROM OUR PARLIAMENTARY CORRESPONDENT]

In the House of Lords on April 2nd the motion for the Notification of Poverty Bill, moved by Lord Stanley of Alderley, was negatived. The Criminal Lunatics (Scotland) Bill was read the third time, and passed.

The House of Commons was occupied this week with Civil Estimates and the committee stage of the Government of India Bill. The business of the House of Lords included the Voluntary Hospitals (Paying Patients) Bill.

The Parliamentary Medical Committee met at the House of Commons on April 1st, Sir Francis Fremantle presiding. Discussion arose about certain recent propaganda against serum therapy. It was reported that the Ministry of Health did not propose at present to take action about the price of insulin. The Medical Committee also decided to take no further action in the matter. A letter was received from the council of the Association of Chemical Pathologists in favour of systematic provision for persons under the National Health Insurance Acts of facilities for laboratory examinations. The committee agreed that Sir Francis Fremantle should explore the matter with the Ministry of Health.

At a meeting of the Conservative M.P.s' Agricultural Committee, at the House of Commons on April 2nd, Sir Francis Fremantle submitted a letter from Dr. Elliot on the propriety of making compulsorily notifiable the occurrence of contagious abortion in cattle. Dr. Elliot was of opinion that notification of premature calving would be useless, unless accompanied by veterinary inquiry and also by restrictions on the movement of any cow which had aborted and of its calf. Even such measures would be inadequate, and appreciable reduction of the disease could only be secured by controlling the movement of infected cows and heifers in diseased herds, even if they had not aborted. Dr. Elliot held that any general order on such lines under the Diseases of Animals Acts would not be supported by the farming community. He added that important progress had been made by the committee on *B. abortus* infection in animals and man, appointed in 1932 by the Agricultural Research Council in consultation with the Medical Research Council. The committee was concentrating on devising methods for the eradication of the disease. In the meantime the Ministry of Agriculture recommended that in herds where the percentage of abortion was not high the disease should be controlled by hygienic measures, accompanied by the agglutination test for the discovery of infected animals. Where infection was heavy the Ministry advised the immunization of all non-pregnant animals with live vaccine from the Ministry's laboratory at Weybridge.

During the debate on the Army Estimates, on March 18th, Sir Francis Fremantle asked why the establishment of officers in the R.A.M.C. had been reduced. Replying by letter, under date April 2nd, Mr. Douglas Hacking contended that there had been no reduction in the actual number of medical officers employed, and no steps had been taken to retire medical officers compulsorily as a result of the reduction in establishment. The reduction of seventy-two medical officers had been achieved by elimination of thirty posts which did not provide an adequate amount of professional work, also by reduction of thirty-two in the number of officers attending courses of instruction, and of ten posts in those allowed for reliefs and miscellaneous.

In the House of Commons on April 2nd the Vagrancy Bill passed through committee, and was read the third time.

Malaria in Ceylon

In reply to Sir A. Beit, on March 27th, Sir P. CUNLIFFE-LISTER said that the Governor of Ceylon had informed him that the Medical Department took prompt steps to deal with the outbreak of malaria in November last, which was at its worst in January, and since then has been on the decline. Such steps as were practicable were being taken against a recrudescence, and arrangements were being made for the visit in the near future of an officer specially experienced in malaria work to investigate the outbreak and advise on the situation. The State Council had appointed a special Commissioner to co-ordinate relief measures. It had voted Rs.1,500,000, apart from sums for medical and other measures, to be administered by him for the relief of distress caused by the outbreak and the food shortage. The organization of the Ceylon Medical Department for the distribution of quinine and for the provision of facilities for treatment had been commended by an expert in tropical medicine who recently visited Ceylon.

Doctors and the Speed Limit

On March 27th Mr. GLOSSOP asked the Home Secretary if he would instruct the police that doctors on their way to urgent cases were not to be prosecuted for exceeding the 30 miles an hour speed limit, and if the same exemption would be granted to veterinary surgeons. Sir JOHN GILMOUR said that the question whether there should be an exemption from the speed limit in the case of vehicles used by members of the medical profession when attending urgent cases was discussed when the Road Traffic Act, 1934, was passing through Parliament, but it was decided that it would not be practicable to allow this exemption. It would be equally impracticable to place on the police the responsibility for assessing the degree of urgency when considering the question of instituting proceedings, and in his view the only satisfactory solution was to leave the matter to the courts.

Maternal Mortality: A White Paper Promised

Replying, on March 28th, to Captain Elliston, Sir HILTON YOUNG said his attention had been drawn to the necessity for special local study of the causal factors of the continuing maternal mortality. Such special study had been continuously in progress in the Ministry of Health. It was the main object of the special committee on maternal mortality appointed by one of his predecessors in 1928. He had recently carried the matter further as regards the local aspect by calling for special reports from the medical officers of health in the areas where the incidence was, or had been, most marked. He had also arranged for thorough local investigation of the problem in those areas and, for purposes of comparison, in other areas, by officers of the Ministry. He had secured the services of an eminent obstetrician to advise on the reports of those officers. Where immediate specific steps were recommended, he brought the matter to the notice of the authority for action accordingly. When all the reports were available he proposed to consider the position as a whole and to lay a White Paper on the subject before the House.

Replying to Sir F. Fremantle, Sir HILTON YOUNG said he could not give a date when the White Paper might be expected, because it depended on the completion of intensive local investigations. Replying to the Countess of Iveagh, the right hon. gentleman said that the possibility of making compulsory a confidential notification of maternal deaths in all areas had been under consideration, but at present the purpose was effectively served by the voluntary information which he got in response to his appeal to the local authorities.

Milk and Infectious Disease

On March 28th Sir HILTON YOUNG, replying to Sir A. Wilson, declined to draw the attention of medical officers of health to the statement in the report of the Cattle Diseases Committee that the available evidence as to the importance of milk in the spread of infectious disease was not conclusive,

and that the number of cases of scarlet fever traced to milk was only 2,500 to 3,000 out of some 2,000,000 cases. Sir Hilton Young said that even if it were established that only 2,500 to 3,000 cases of scarlet fever in the last twenty years were attributable to milk, he would still consider it important that milk should be pasteurized to prevent such cases in the future. But, in fact, the Committee on Cattle Diseases considered that infection through milk might be more extensive than appeared at first sight, the reason being that it was only where a large number of cases occurred that the milk supply was likely to be suspected. Also, scarlet fever was not the only milk-borne disease which could be prevented by pasteurization. Sir F. FREMANTLE asked if the actual figures did not prove the value of the great attention which medical officers had given to milk in the past twenty years, and Sir HILTON YOUNG replied: Undoubtedly so. Sir A. WILSON further asked if the Minister would draw the attention of medical officers of health to the statement, on page 28 of the report of the Cattle Diseases Committee, that the most serious outbreak of disease brought to its notice, involving over 500 deaths, was traced to pasteurized milk infected by a human carrier. The MINISTER replied that the outbreak in question occurred in Canada, and he understood that, while it was not found possible to determine exactly how the milk became infected, the weight of evidence was to the effect that the infection was introduced before the milk reached the pasteurizing depot, and that much of the milk was distributed without having been submitted to the pasteurizing process. He saw no reason to call the special attention of medical officers of health to the matter.

Hospital Maternity Bookings

On March 28th Mr. PARKINSON asked the Minister of Health whether, arising out of the Manchester case of Mollie Taylor, in which it was found that the porter of the hospital took all bookings for maternity cases, and that those bookings bore no relation to the number of beds available, he would inquire as to how many cases there were in which this practice of employing a porter obtained, and would require that some qualified person, such as a lady almoner, should book the cases in relation to the number of beds available. Sir HILTON YOUNG said that many maternity hospitals were outside his jurisdiction, but he was advised that the method of booking referred to was quite unusual. His medical officers inquired into the matter at the hospitals which they visited. The published reports on Mrs. Taylor's case contained a recommendation that the system of booking at the hospital concerned should be placed under the charge of a responsible officer.

Scarlet Fever in Buckinghamshire

On March 28th Sir FRANCIS FREMANTLE asked if the Minister of Health could make a further statement regarding the recent outbreaks of scarlet fever at Denham and Iver, Bucks, the number of cases and of deaths, the origin of the infection, and the administrative conclusions involved, with a view to the prevention of such outbreaks in future. Sir HILTON YOUNG said that up to the present ninety cases had been reported in which the signs and symptoms were sufficiently well defined to justify a diagnosis of scarlet fever, but there had been, in addition, a large number of mild cases in which the only clinical symptoms were sore throat, vomiting, or slight fever. There had been two deaths. As regards the origin of the infection, he had nothing to add to an answer given on this subject on March 21st. The outbreak confirmed the conclusion that it was practically impossible to avoid similar occurrences otherwise than by efficient pasteurization, or some equally effective form of heat treatment of the milk supply.

Birmingham Infectious Diseases Hospital

On March 28th Sir HILTON YOUNG informed Mr. Groves that according to information which he had received from the Birmingham local authority, classified records of illnesses of nurses in the Birmingham City Infectious Diseases Hospital were available for 1932 and 1933, and were set out in the medical officer's annual reports for those years. Classified records for 1934 were not yet available. The Minister also told Mr. Groves that 2,162 cases were admitted in 1930 to the Birmingham City Infectious Diseases Hospital as diphtheria, and 744 were diagnosed as being some other disease. In 1931, of 1,728 cases admitted as diphtheria, 690 were diagnosed as of other diseases. In 1932, of 1,156 cases admitted as diphtheria, 609 were diagnosed as of some other disease, and 303 were rediagnosed as tonsillitis. The figures for 1933 were 788 cases admitted, and 441 diagnosed as some other disease, while 285 were rediagnosed as tonsillitis. In 1934 cases admitted numbered 1,551, but figures as to the diagnosis were not yet available.

The Lunacy Acts

On April 1st Dr. O'DONOVAN asked the Minister of Health if representations had reached his Department or the Board of Control that the requirements of Section 49 of the Lunacy Act, 1890, had been altered within the last ten years to the detriment of the friends or relatives who desire to secure the discharge of a patient; that previous to that date the two doctors appointed to examine a patient paid one visit each and reported, but now they were required to go together on two visits, thus doubling the cost in fees; and, whether in view of the fact that such an increased cost was prohibitive to the poor, he will look into this and see if it was possible to return to the practice previously obtaining. Sir HILTON YOUNG said the requirements of the section referred to had not been altered, but some years ago the Board of Control, after careful consideration, expressed the opinion that the section required that the two medical practitioners should examine the patient together on two separate occasions. Since 1928 the Board had received two representations in the matter. The point had been noted for consideration in the event of amending legislation being introduced.

Pay-beds in Voluntary Hospitals

In the House of Lords, on April 2nd, Lord LUKE moved the second reading of the Voluntary Hospitals (Paying Patients) Bill, which enables hospitals to provide accommodation for pay-beds. Such beds, the noble lord explained, were not to be provided at the expense of, or reduction in, the number of ordinary beds. Patients in the pay-beds would be those whom a hospital almoner would not usually accept in the ordinary wards, but who could not afford the cost of private treatment in their homes, and urgently required the assistance of a hospital, with all its specialist treatment. The provision of pay-beds in recent years had become recognized as a customary part of the function of voluntary hospitals. The *raison d'être* of the Bill was that some hospitals, in order to provide pay-beds, had recently obtained private Acts, and others wishing to do so must adopt the same procedure, which must cost at least £500. The Bill would provide a uniform method of removing such anomalies, while safeguarding the interests of the sick poor. The Bill embodied the considered views of the King Edward Hospital Fund and the Hospitals Association. Lord SANDERSON said that the Opposition had certain misgivings about the Bill. It was very anxious that it should not be the means of cutting down the services for poor people who could not afford to pay. It did not want the waiting lists in the hospitals to become longer or to run the risk of hospital treatment being cut short because of the pressure of the waiting list. The Opposition was very dissatisfied with the hospital system, or lack of system, and wanted to see all the hospitals taken over by the municipalities, publicly controlled, and kept open for free treatment for all who needed it. They should be supported out of the rates. The EARL OF DUDLEY, speaking as chairman of one of the large voluntary hospitals in London, supported the Bill. He said he hoped that it would not interfere with the powers of committees of management to charge varying fees in hospitals where they had the power to charge for in-patients. He had heard it said that it was the intention of the Charity Commissioners to issue an order limiting the amount to be charged to seven guineas. That would be a hardship on the lower-paying beds. In the hospital which he controlled they were able to charge as high as ten guineas for single rooms,

but if they were to limit their charges to seven guineas they would have to raise the fees of the lower-charged beds, because the ten-guinea beds naturally covered to a large extent the charges made for the beds of lower cost. Viscount HAILSHAM said that Lord Luke had made out a case for the desirability of having some beds available for that class of persons who were willing to make some payment towards the necessary expenses of receiving treatment, but were unable to pay the fees of a nursing home. The Government had considered the point made by Lord Sanderson, and had reached the view that it was right to provide pay-bed accommodation, but that safeguards should be insisted on to ensure two things: that the property and funds of a hospital settled on it for the benefit of the sick poor were protected and prevented from being diverted for any other purpose; and, secondly, that the accommodation which would otherwise be available in the hospital should not be restricted, and that there should be no risk of preventing poor people from having the amount of accommodation which would otherwise be available for them. While the Government would support the second reading of the Bill, it would feel it its duty to move amendments proposing specific safeguards with regard to the funds and property of the hospitals and dealing with other matters. He did not agree with Lord Sanderson's preference for a municipal rather than a voluntary system.

The Bill was read a second time.

Diphtheria Immunization

Mr. GROVES, on April 2nd, asked the Minister of Health, with regard to the Medical Research Council's Special Report No. 195 (pages 72 and 83), by whose permission and with whose approval Case 2/29 had received six injections of toxoid and six further injections of floccules and numerous Schick tests in sixteen months; and whether, if permission was given to the officials in charge to administer diphtheria prophylactics, the permission was intended to cover such a procedure. Sir B. EYRES-MONSELL said that permission to administer prophylactics was in all cases obtained from the boys' parents or guardians. As the report showed, Case 2/29 was a most difficult and lengthy one, but it was, of course, necessary to continue injections until immunity from diphtheria had been produced.

On March 18th Mr. GROVES asked the Minister of Health, in reference to page 77 of the report, whether the Schick testing and blood testing of six boys at Greenwich Hospital School suffering from diphtheria, who were Schick-tested and bled during their illness, took the usual seven to fourteen days before the Schick test could be determined; and what curative measures were taken. Sir B. EYRES-MONSELL, who replied, said that in these cases no record was made of exact times taken, but in the majority of cases the result could be determined in twenty-four hours. The boys were treated with the greatest care and attention, with rest in bed and local application to the throat, and they were all practically well in three days. These tests, which have been in general use throughout the world for many years, and the practical value of which might be accepted, could not properly be called experimental, as was suggested in the question.

On April 2nd Mr. GROVES also asked the Minister of Health whether any charge was made for the antitoxin estimations at the Greenwich Hospital School by outside commercial institutions; and whether, as this institution had a financial interest in diphtheria immunization, it was still proposed to entrust such investigations to it. Sir B. EYRES-MONSELL said that no charge was made for the antitoxin estimations, which were carried out gratuitously by eminent bacteriologists. At present no further estimations were contemplated.

Births in Scotland.—On March 27th Sir GODFREY COLLINS told Mr. Leonard that the detailed examination of the data collected in connexion with the inquiry into the circumstances attending births in Scotland had now been completed, and the report would shortly be submitted to the Scientific Advisory Committee. He hoped that it would be possible to publish the report in about two months' time, after the committee had had an opportunity of considering it.

Medical News

The House of the British Medical Association, including the Library, will be closed for the Easter holiday from 6 p.m. on Thursday, April 18th, until 9 a.m. on Tuesday, April 23rd.

The 115th annual general meeting of the Hunterian Society will be held at Simpson's Restaurant, Cheapside, on Monday, April 8th, at 8.30 p.m., after dinner at 7.15. At the conclusion of formal business Lieut.-Colonel R. H. Elliot will give a short talk on snakes and their bites, illustrated by lantern slides and a film. Non-medical guests, including ladies, may be invited.

A meeting of the Southern Branch of the Association of Certifying Factory Surgeons (Incorporated) will be held at B.M.A. House, Tavistock Square, W.C., on Friday, April 12th, at 3.30 p.m., when Dr. S. A. Henry will read a paper on "Industrial Medicine." A cordial invitation to attend is extended to all factory surgeons.

Discussions on "Administration of the Housing Act, 1930," to be opened by Mr. Colin Campbell, and on "The Hospital Services," to be opened by Dr. D. C. Lamont, medical officer of health, Burnley, are included in the programme of the sessional meeting of the Royal Sanitary Institute at Burnley, on April 12th and 13th.

An intensive course in laryngology, rhinology, and otology will be held at the Central London Throat, Nose and Ear Hospital, Gray's Inn Road, W.C.1, from April 29th to May 25th. The course, which is especially suitable for D.L.O. students, will include an anatomy and physiology, and operative surgery classes, a practical course in peroral endoscopy, and a course in pathology and bacteriology. Particulars of fees, etc., may be had from the secretary.

The Fellowship of Medicine (1, Wimpole Street, W.) announces that a course in proctology will be given at St. Mark's Hospital from April 8th to 13th, occupying the whole of each day. A course in psychological medicine will be given at Maudesley Hospital from April 23rd to May 31st. Other forthcoming courses include: dermatology, at St. John's Hospital, April 29th to June 1st; medicine, surgery, and gynaecology, at Royal Waterloo Hospital, April 29th to May 11th; thoracic surgery, at Brompton Hospital, May 13th to 18th; week-end courses in cardiology, at Victoria Park Hospital, May 4th and 5th; chest diseases, at Brompton Hospital, May 11th and 12th. A panel of clinical teachers is available.

The fourth International Hospital Congress will be held in Rome from May 5th to 18th. It will be preceded by visits to the hospitals of Milan, Genoa, and Florence, and followed by excursions to Naples, Sicily, and Tripolitania.

The first Congress of the History of Greek Medicine, organized by the Hellenic Society of the History of Medicine, will be held at Athens from May 8th to 11th.

The forty-eighth Congress of the Société Française d'Ophthalmologie will be held in Paris from May 13th to 18th, when the chief subjects for discussion will be capsulo-lenticular extraction of cataract, introduced by Dr. de Saint-Martin of Toulouse, and treatment of detachment of the retina. Further information can be obtained from Dr. René Onfray, 6, Avenue de la Motte-Picquet, Paris VIIe.

The Minister of Health, Sir E. Hilton Young, will open the new house of the British Dental Association at 13, Hill Street, Berkeley Square, W., on Friday, April 12th, at 3 p.m.

The *Yorkshire Post* announces that gifts of £50,000 and £23,000 in 2½ per cent. Consols have been made by an anonymous donor to the Sheffield Royal Infirmary, and that a new theatre block is to be built in commemoration of the King's Silver Jubilee.

The vital statistics of European officials in East Africa in 1933 have been published in the usual form by the Crown Agents for the Colonies, 4, Millbank, S.W.1 (price 6d).

Letters, Notes, and Answers

All communications in regard to editorial business should be addressed to **The EDITOR, British Medical Journal, B.M.A. House, Tavistock Square, W.C.1.**

ORIGINAL ARTICLES and LETTERS forwarded for publication are understood to be offered to the *British Medical Journal* alone unless the contrary be stated. Correspondents who wish notice to be taken of their communications should authenticate them with their names, not necessarily for publication.

Authors desiring REPRINTS of their articles published in the *British Medical Journal* must communicate with the Financial Secretary and Business Manager, British Medical Association House, Tavistock Square, W.C.1, on receipt of proofs. Authors over-seas should indicate on MSS. if reprints are required, as proofs are not sent abroad.

All communications with reference to ADVERTISEMENTS, as well as orders for copies of the *Journal*, should be addressed to the Financial Secretary and Business Manager.

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QUERIES AND ANSWERS

Climate for Chronic Arthritis

"S. W." writes: Can anyone recommend the best localities in England for a middle-aged, active lady suffering from rheumatic gout? It came on shortly after she and her husband went to Devonshire in the late autumn. She had never had anything but rare and minor indications of any rheumatic diathesis previously, and has exceptionally good health otherwise. They were obliged to leave Devonshire as she had great stiffness and pain in all the big joints, combined with some fibrositis. A double course of Vichy douche treatment has given very great relief, and the question now arises of the best place to live in, as they are not tied in any way.

⁂ We have referred this query to Dr. R. Fortescue Fox, who suggests that "S. W.'s" case may possibly prove a form of climacteric arthritis, which is usually amenable to judicious treatment, including a periodical course of baths. Cold and exposed and humid districts should be avoided, especially in autumn and winter, but most parts of southern England are suitable, especially the hinterland (a mile or more from the coast) of Sussex and Hants.

Defective Development

Dr. L. ROWAN writes: I shall be grateful for suggestions as to treatment of a boy aged 17 years (child of an exceptionally tall and well-built father, and a mother who is also well over the average in size), who weighs only 4 st. 12 lb. and is 4 ft 9 in. high, and yet normally built, with no deformity of limbs or other indication of rickets; nor is it a question of syphilis, either acquired or inherited. All the organs are sound, and the boy is of quite the average of intelligence for a lad of his years. Owing to the defective muscle development, however, the joints of the limbs, in contrast with the girth of the muscles above and below, stand out so markedly as to produce an appearance of deformity. The boy is anxious to be at work, and does do a certain amount of light work every day, but towards evening feels "done up," and often has to lie down. He was treated, until a couple of years ago, with cod-liver oil, extracts of malt, etc., but they did not appear to help, and he gave them up. Perhaps some reader might be able to suggest which (if any) of the modern methods in the endocrine line would be worth trying. The boy has not been long under my care, and I have not tried thyroid or other glandular extract, pending possible help from those with more experience. I may add that the thyroid gland is so small as to be almost absent. There seems no abnormality of fauces or tonsils. He has never had any serious ailment.

Chronic Nasal Discharge in Children

Dr. A. G. WELLS ("Weybrooks," Medmenham, near Marlow, Bucks) writes: In reply to "G. P." (Surrey) I have found that diastolization is the best form of treatment for this condition in young children. Should "G. P." not be familiar with this I shall be happy to give him further information, and, if necessary, demonstration, if he will communicate with me.

Income Tax

Cost of Locumtenent

"SUBURB" has a practice in "a lower middle-class district"; he considers £5 a week a reasonable amount to charge for the accommodation and board of the locumtenent, but the inspector of taxes considers £1 a week the maximum due. No deduction is made for the "rent and attendance of the living portion of the premises."

⁂ We agree with "Suburb" that the estimate of cost must have regard to the nature and cost of the board, etc., actually provided rather than to the nature of the practice. It is almost impossible to suggest an actual figure; at a guess the amount allowable would usually be somewhere about half-way between the above figures. One way of testing the estimate is to arrive at the total cost of the domestic establishment and allocate a reasonable proportion to the locumtenent, taking into account the fact that he has a separate room and any other material circumstances.

LETTERS, NOTES, ETC.

Emblem for Doctors' Cars?

Dr. HARRY SUGARE (Leeds) writes: For the information of Dr. Edward S. Jackson (March 30th, p. 688) I would like to inform him and those interested that, in reply to a letter of mine on the same subject, I have received a communication from the R.A.C., who inform me that the question of the position of doctors under the thirty-mile-speed limit was very fully debated in the committee stage of the Bill. An amendment was put down in Clause 3 to exempt vehicles used for medical purposes. Mr. Oliver, then Minister of Transport, resisted the amendment. It was subsequently suggested that doctors who were engaged on very urgent calls should carry a distinguishing mark on the car. Mr. Stanley stated: "That would only meet the case if you restricted the use of the sign to an emergency call. I am sure that the hon. member [Mr. Llewellyn-Jones] will realize that in practice it would be impossible. If you allowed a doctor to have this distinguishing mark you could not prevent him from using it at all times and for all purposes, and it would, in fact, take him outside the scope of the speed limit altogether, which, I think, the committee do not want to see done." I feel sure doctors would not abuse this privilege if it were granted.

"J. E. K." writes: Some time ago I inquired if the B.M.A. had considered taking up the question of a concession for the doctor on urgent duty. The reply was that the matter had not been considered. Since the speed limit was introduced I have hastened to a post-partum haemorrhage and to a child choking, and so far the police have not noticed that I broke the speed limit. I was justified in hurrying. I suggest, Sir, that the B.M.A. must consider this matter. Even the man in the street expects it.

A new shilling monthly magazine, *Family*, makes its bow this month. It is edited by a father-of-seven from his own family circle, and its aim is to reflect the vitality and the freshness of the surroundings in which it has been evolved. The contents of the first number include a note on health and the home, by Dr. Stanford Read. *Family* is published by W. E. Baxter Ltd., Lewes, Sussex.

Vacancies

Notifications of offices vacant in universities, medical colleges, and of vacant resident and other appointments at hospitals, will be found at pages 47, 48, 49, 50, 51, 52, 53, and 56 of our advertisement columns, and advertisements as to partnerships, assistantships, and locumtenencies at pages 54 and 55.

A short summary of vacant posts notified in the advertisement columns appears in the *Supplement* at page 132.